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THE NEW ROYAL WARRANT FOR THE ARMY VETERINARY DEPARTMENT.

BY "UBIQUE."

IN the June number of the *Veterinary Journal* appeared the last Royal Warrant for the Veterinary Department of the Army, with an editorial thereupon which, taken as a whole, expresses satisfaction with its terms. Here the writer proposes to review the prospects of a veterinary surgeon who entered the army previous to the publication of this warrant, and under the operation of the warrant of 1859, as compared with his present prospects, up to the period of service which qualifies him for the rank of Inspecting Veterinary Surgeon; and it will be seen that the advantages offered by the new warrant are not so great as are apparent at first sight.

Under the warrant of 1859, a veterinary surgeon received on appointment 10*s.* a day, and—with the exception of those posted to cavalry regiments—forage for two horses at the rate of 1*s.* 10*d.* a day each, making a daily total of 13*s.* 8*d.* a day. After five years' service, an increase of 1*s.* 6*d.* a day was received, and forage as before: total, 15*s.* 2*d.*; after ten years', a further increase of 1*s.* 6*d.*: total, 16*s.* 8*d.*; and after fifteen years'.

another increase of 1*s.* a day. So that at fifteen years' service, if not promoted to first-class rank, a veterinary surgeon received 17*s.* 8*d.* a day. It was very seldom, however, that a man of average abilities had to wait so long without promotion; although, as pointed out in the editorial alluded to, some were unfortunate through no fault of their own. Under the 1859 warrant, a veterinary surgeon was considered eligible for promotion to first-class rank after five years' service. The writer, however, does not know of a case in which promotion was received at so short a service; but after eight or ten years it was not at all uncommon.

For convenience, we will take the case of an officer promoted after ten years' service. A veterinary surgeon on promotion to first-class at this period received 14*s.* 6*d.* a day pay, with forage as before—making a daily total of 18*s.* 2*d.* a day. After fifteen years' total service, there was an increase of 1*s.*—making his daily pay and allowances 19*s.* 2*d.*

Now let us look at the other side of the question. Under the new warrant, veterinary surgeons are allowed forage for *the number of horses necessarily kept for duty*, which will be made to mean *one* and no more (except in India and on active service), the forage allowance amounting to 1*s.* 10*d.* a day. Now, in the tables below will be seen a comparison between the total daily pay and allowances, as they are under the new warrant, and were under the old—the ten years' rule not being included.

New Warrant.

Rank.	After fifteen years' total service.			10			5			Under 5.		
	Pay.	Forage.	Total.	Pay.	Forage.	Total.	Pay.	Forage.	Total.	Pay.	Forage.	Total.
	s. d.	*s. d.*	*s. d.*	*s. d.*	*s. d.*	*s. d.*	*s. d.*	*s. d.*	*s. d.*	*s. d.*	*s. d.*	*s. d.*
Vet. Surgeon 1st Class	16 0	1 10	17 10									
Vet. Surgeon.				14 6	1 10	16 4	12 0	1 10	13 10	10 0	1 10	11 10

It will be noticed that at fifteen years' service, the first-class

veterinary surgeon has not arrived at his first increase of pay in that rank.

Old Warrant.

Rank.	After fifteen years' total service.			10						Under 5.		
	Pay	Forage.	Total.	Pay.	Forage.	Total.	Pay.	Forage.	Total.	Pay.	Forage.	Total.
	s. d.	*s. d.*	*s. d.*	*s. d.*	*s. d.*	*s. d.*	*s. d.*	*s. d.*	*s. d.*	*s. d.*	*s. d.*	*s. d.*
Vet. Surgeon 1st Class.	15 6	3 8	19 2	14 6	3 8	18 2	12 6	3 8	16 2			
Vet. Surgeon.	14 0	3 8	17 8	13 0	3 8	16 8	11 6	3 8	15 2	10 0	3 8	13 8

In the above it will be seen that the pay and allowances under the old warrant were really much better than they now are, up to fifteen years' service; after that period the new warrant shows a great improvement on the old one. But why should the junior members of the Department be so niggardly treated? and what must be the disappointment of those officers immediately below those last promoted to first-class rank, to find that their chances of promotion, instead of being increased, have been lessened and themselves thrown back two or three years? Having seen those just above them promoted after ten years, they not unreasonably hoped that they too would have their turn at about a similar period; and it is too absurd to term a warrant one for the improvement in pay and position of officers of a department, when fully one-third of the members of that department become, under its operation, worse off than they were before, with regard to pay and allowances inclusive, while as regards relative rank they are just as they were. Many years ago, when the writer was in England, and a new warrant was spoken of, one of its leading features was to have been the conferring of the relative rank of captain after six years' service, as is now done in the Medical Department. This, however, we do not ask for, much as we may wish for it. But why should we have to wait twelve years for what is obtained by the surgeon after six, and the combatant officer as a rule after ten or eleven? Combatant officers enter the service at an average age of 19

or 20; veterinary surgeons at 21 or 22; consequently, when the veterinary surgeon obtains his promotion, he finds that the subaltern who entered the service with him has received his captaincy two or three years before. If credit were given to the professional officer for the difference in age, and promotion calculated accordingly, then the veterinary surgeon would obtain the relative rank of captain after about eight years' service. Now, this could be done at a comparatively trifling expense to Government, by doing away with all departmental distinctions below the rank of Inspecting Veterinary Surgeon, giving relative rank according to service. In the table below is a not very extravagant idea of what would go a long way towards giving satisfaction:—

Rank.	Total Service.	Relative Rank.
Inspecting Vet. Surgeon	After 20 years	Lieutenant-Colonel.
,, ,, ,,	Under 20 years	Major, according to date of commission.
Veterinary Surgeon	After 15 years	Major, but junior of the rank.
,, ,,	After 8 years	Captain.
,, ,,	Under 8 years	Lieutenant.

The pay would, of course, have to be slightly altered in scale; although the total amount received by a veterinary surgeon during his service would not exceed that under the present arrangements. Retirement might remain as it is, or very nearly so. Promotion to Inspecting Veterinary Surgeon should be given by selection after fifteen years' service, much in the same manner as promotion to Deputy Surgeon General is now given in the Medical Department. Forage should be given as before, with the exception of the deduction from the pay of those serving in cavalry regiments, which was too monstrous an injustice ever to be repeated, let us hope. It is quite impossible for all veterinary surgeons to attain the higher rank; consequently, an increase of relative rank should be given on reaching a fair seniority in the service (this rank is given in the Medical Department after twelve years); and the relative rank of captain given to veterinary surgeons of eight years' standing, is no more than they ought reasonably to expect.

Now a few words with regard to those who are about to enter, or who may have entered, the service under the new ten years' rule. As pointed out by a writer in the *Army and Navy Gazette* of June 8th—although these gentlemen are promised £250 a year while serving in England, they will pass very few of those ten years at home; and for at least five or six years, while they are in India, will draw exactly the same pay, etc., as they would under the old warrant. No alteration in the pay of the ten years' surgeons has been made by the Indian Government, nor will any be made in the case of their veterinary brethren.

The writer cannot concur in the sanguine views expressed by the *Veterinary Journal** as to the probability of obtaining candidates under the new rules; and it will be seen that the new warrant will not suffice to procure numbers enough to raise the Department to its proper strength. There are now (9th July) more than twenty vacancies to fill, which will test the seductiveness of the new arrangement severely.

It is greatly to be deplored that so good a warrant as this last might have been, should have been utterly spoiled by a want of consideration for the junior members of the Department. The writer, however, feels assured that no blame can be attributed to the head of the Army Veterinary Department, who has always shown that he has the welfare of all grades impartially at heart; and the Department may rest assured that consideration of the claims of the juniors will not long be withheld.

ECZEMA OR OBSTINATE SKIN DISEASE OF THE HORSE.

BY FRANCIS WALKER, M.R.C.V.S., 7TH HUSSARS.

HAVING read Mr. Butters' communication on "Obstinate Skin Disease of the Horse" in the *Veterinary Journal* for September, I forward a few remarks on some cases of the same disease which your correspondent has so well and graphically described.

* June issue—Editorial.

The disease, I have no doubt, is Eczema, which may be, and is very frequently, mistaken for Mange.

The first case, a troop horse of the regiment, came under my notice in May 1876, when stationed in Edinburgh. The symptoms were the same as those described at p. 156 of the *Journal;* but, in addition, there was considerable corrugation of the skin in various parts of the body, though in all cases on each side of the neck and shoulders.

I have had eleven cases of this disease under my care—some of them admitted twice, others three times, to the infirmary; and though I made trial of every recognised mode of treatment, both externally and internally, I could not succeed in eradicating the disease.

During the winter of 1877-78, and at the time of the recent outbreak of the frightful disease affecting most horses in Dublin, I was frequently in consultation with T. D. Lambert, Esq., the eminent veterinarian of that city, respecting his most valuable patients, and often the worst cases; and being one day at his recently erected suburban infirmary, I observed a pony which had been dressed for supposed incurable Mange. Upon inquiry, I found that crude paraffin oil had been used; and having tried so many remedies in the vain endeavour to cure my patients, I determined to make trial of the paraffin oil in one of the worst cases.

I had the patient thoroughly cleaned and washed, and then under my superintendence the oil was applied, so that no part of the body was left untouched. The oil being thoroughly rubbed in with the palm of the hand, the horse was securely fastened for twenty-four hours, and then allowed to walk about a loose-box. The horse has never rubbed against wall or stall-post since; but to my surprise, about a fortnight after the application, the hair began to fall off in large patches or flakes, and to facilitate the removal a soft dandy brush was lightly used.

The body was thus totally denuded of hair, and the skin, after one washing, was beautifully soft and clean.

During the time the new hair was growing, the patient was warmly clothed and lunged round a circle for a sufficient length

of time to produce slight perspiration; he was then taken back to the box and rubbed down with a dry wisp.

The only medicine administered was an occasional draught composed of magnesia sulph. ʒiv., zingib. tinct. ʒij.

I have since treated many cases in like manner, and all with the happiest result.

On reading Mr. Butters' communication, I at once wrote to that gentleman describing and recommending a trial of the same treatment, and asking him to let me know the result. This he has in the most courteous manner promised.

So successful has this treatment been, that the Principal Veterinary Surgeon to the Forces, when making his annual inspection in August last, did not observe any traces of the disease; though the horses which had been discharged cured were ranked past in single file (uncovered and led) on a bright sunny day.

During the progress of the disease, many times were microscopical examinations made with the view of discovering parasites, either in the hair, scurf or skin. Failing to discover any kind of parasite, a portion of the skin about half an inch square was, at the suggestion of the Principal Veterinary Surgeon, and at the request of Dr. Thin, the eminent microscopist, forwarded to that gentleman for dissection and examination.

I am informed he pronounces it to be Acne, or Acme (from the Greek ἀκμή), which means an inflammatory state of the sebaceous follicles, caused by the accumulation of the sebaceous matter.

Great diversity of opinion existing as to the contagious or non-contagious nature of this disease, I determined to experimentalise, and placed a four-year-old colt of my own, which I had just purchased, in the stable with four affected horses.

The same articles were used in the grooming of all five horses, for the space of six weeks; but up to the present time—ten months—there is no apparent indication of the disease existing in my own horse.

I am therefore inclined to enlist myself in the ranks of those who deem it non-contagious, amongst whom I may name that

eminent author, Professor Williams, of the New Veterinary College, Edinburgh.

At one time I thought the forage (not always of the best) might, through the digestive system, be the approximate cause in the production of this disease; but frequent microscopical examinations of the oats, bran and hay tended to dispel this illusion. Besides, there was the fact of all the horses in the regiment—400—being fed with the same forage.

This disease is very prevalent in many parts of India, but I never saw any cases during my stay in that country, from October 1866 to April 1870. Should any of our brethren who may be luxuriating in that sunny clime be familiar with the malady, I trust they will favour us with their opinions on the subject; as it is only by the interchange of ideas on matters of this kind that we are able to arrive at any definite conclusions.

ECZEMA SIMPLEX, HUMID TETTER (W. WILLIAMS)—PRURITIC ECZEMA, SPURIOUS MANGE (MACGILLIVRAY)—AS AFFECTING HORSES, DOGS, ETC.

BY A. E. MACGILLIVRAY, V.S., BANFF, N.B.

THE above peculiar affection has been uncommonly prevalent in my practice during the past summer, and I have had to treat some very intractable cases; so that, on reading the communication of Mr. L. Butters and the editorial *addendum* in the September *Journal*, I at once resolved to offer a few remarks on the subject.

For a long time I considered this skin affection a variety of "Mange"; but although often searching diligently for the causative parasite, I have never yet discovered a single individual *acarus*. So I am compelled to admit its non-parasitical nature. It cannot be denied, however, that it bears in many respects a most striking resemblance to Mange, the only *desiderata* being the absence of the parasite and the (in general) restriction to one animal in a stable; hence it is neither parasitical nor contagious—two very necessary factors in diagnosing Mange.

While asserting its great similarity to Mange, and yet admitting its non-parasitical and non-contagious nature, it is a remarkable fact that, in so far as I am concerned, the only infallible remedy (if I may be allowed the expression) has been, and is, a strong *mange* or *parasiticide* ointment. I have invariably found that the proper application of this ointment affords relief, and with one or two applications effects a cure. As a matter of course, there are other things to attend to besides the mere application of the ointment.

I need not here enter into any details as to the analogy existing between the symptoms seen in *true* and *spurious* Mange, as it is likely the Editor of the *Journal* will do so; suffice it to say that, with the exceptions above mentioned, they are as nearly as possible alike; and this not only in the horse, but also in the dog, and occasionally in sheep and pigs. During the past summer my experience has been confined to the two former, and more especially horses, from the foal to the aged animal. My two most inveterate cases were a young foal and a middle-aged very fat draught gelding; this gelding was about the most intractable case I have ever met with. As to the foal, its mother was affected with *Eczema Impetiginodes* in all four legs, and I could scarcely help inferring from this that there was some connexion between the disease in the mother and that in the offspring. The poor little creature had no peace, and was continually rubbing; there were a great many bare spots all over the body; in some places a minute eruption; in others very small crusts or scars, with wrinkling or corrugating of the skin.

To show the nature and method of my treatment, I shall quote the details of a most virulent case. A farmer, who resides some six miles out, purchased a half-bred horse on a Friday. On the following Sunday this horse commenced to *claw* and *rub* himself. On the Monday he was so much worse, and so very irritable and itchy, that the owner resolved to take him into town and show him to me. Accordingly he was put into a carriage and driven in here—over six miles; but before arriving he became very restless, throwing himself from side to side in the vehicle, and it required all the owner's strength

to keep hold of him. I happened to be in the stable-yard on his arrival, and he was in such a furious state that we had to twitch him severely before we could get him unyoked and unharnessed. When unrigged he presented a most pitiful appearance: his skin about the neck, shoulders and sides was one mass of corrugations or large wrinkles; below the saddle and collar the hair was apparently gone, and there was nothing but a red raw surface left. The poor brute was in the utmost distress, dripping with sweat, and heaving at the flanks, etc. Another horse had to be procured to take the carriage and owner home.

The treatment adopted was an aloetic purge, and rub well into the affected parts the following:—

℞. Potassæ subcarb. pulv. . .	℥ss.
Aquæ rosæ fl.	℥j.
Hydrarg. sulphurat. rub. . .	ʒiss.
Adipis suillæ	℥xj.
Sulph. sublim.	ʒiiij.

This was washed off next day with 10 per cent. carbolic soap and warm water; and on the third day a second application of the above ointment was made to all the spots showing any signs of acute disease. The horse also received a course of diuretics, alteratives and tonics, composed of pot. nit., sulph. sublim., and ferri sulph.

In some exceptional cases, where I have had to use the above ointment beyond a second application, I have added to the same one ounce *Olei Picis;* but in general one good rubbing, followed on the third day by a slight touch here and there, has been almost invariably successful,—in fact, I may repeat—infallible!

In one of Mr. Butters' cases, at the request of the owner, a large bleeding was taken from the jugular. This is, in my opinion, not only questionable, but dangerous practice; for I find Phlebitis, and often subsequent obliteration of the jugular, supervene on venesection in such cases. It is a curious fact that, in almost every case of this spurious Mange, the owners are certain to be very anxious to have a little blood drawn; at least I am, I may say, *always* importuned to bleed.

A case in point strikes me. I remember being sent for late one Sunday night to see a valuable Clydesdale stallion which was said to be threatened suffocation from some swelling about his throat. On attending, some four miles off, I found the patient very unwell—immensely swollen about the throat, chiefly on the off side; and on examining that side, I discovered signs of recent blood-letting; and somehow I, quite as a matter of course, as it were, put my finger on the vein, but no vein arose. Phlebitis, and subsequent jugular obliteration, had followed the owner's practice of phlebotomy to relieve the horse during an attack of Pruritic Eczema.

I remember another case in which, at the owner's urgent desire, my assistant bled a spurious Mange patient *after recovery*, and a slight attack of Phlebitis followed.

In conclusion, I can confidently assert that, if the above ointment is carefully applied as directed, and the other treatment attended to, *and saddles and collars lined with leather*, a cure will infallibly follow.

Theoretical Remarks.—While admitting the absence of animal parasites in spurious Mange (*Eczema simplex*), I must be allowed to remark that some authors have chronicled the discovery of vegetable parasites, and have published representations of the appearances of such under the microscope. One writer discovered spores or *Mycelia* of some fungus in *four* out of *six* cases of *Eczema simplex*. It is an open question, however, whether the spores in the four cases were accidental; or their absence in the other two cases was merely a supposition—their presence not being detected by the observer through carelessness or want of proper opportunity.

I bring forward these remarks as to the supposed vegeto-parasitical nature of *Eczema simplex* (or Pruritic Eczema), because a good number of cases which I have been called on to treat have supervened on the use of musty "hair-moulded" harness; and I have in such cases invariably blamed the moulded harness for bringing about the attack of Pruritic Eczema. It cannot be doubted that in such musty "hair-moulded" harness, we have spores and *Mycelia* of all sorts, and in myriads.

It is quite possible, therefore, that a variety of Pruritic Eczema

may exist, having a vegeto-parasitical origin; or, in other words, certain attacks of spurious Mange may be traced to and caused by the spores and *Mycelia* of some unknown fungus.

ECTHYMA.

BY A. E. DALBY, M.R.C.V.S., BURNHAM MARKET, NORFOLK.

IN perusing the *Veterinary Journal* for the month of September, I read with pleasure Mr. Butters' article on "An Obstinate Skin Disease of the Horse." Having had several cases lately of the same description, and in fact some now under treatment, in the practice of Mr. Jas. Wright, M.R.C.V.S., Burnham Market, Norfolk, to whom I am assistant, and thinking what a similarity they possess, and also as the Editor invites any who have studied the disease, and been successful in curing it, to favour the profession with their views, I take the liberty of complying with his request and forwarding the following notes. No doubt there will be a great many different opinions upon this disease, from much abler pens than mine; but, nevertheless, I state what I have found in practice,—and as regards theory, I cannot class the malady under any other name than "Ecthyma," as the different morbid conditions of the skin and symptoms exhibited come within the meaning of that term.

Skin diseases I have always found designated by so many different names; one writer describing a disease and calling it by one name, another by quite a different one, and even placing it in a different class altogether, according to their classification, whether pathologically or artificially.

With regard to this disease, I find it affecting both horses and neat stock; and it matters not whether the animals are in a plethoric or anæmic condition. For instance, I can call to mind lately, two ponies, a cob, a cart filly, and a bullock, which have suffered from this disease; and the condition of these has greatly varied.

The symptoms are exactly as described by Mr. Butters, so they require no further comment.

In neat stock, the lesions of the skin shown are exactly as if the animals were under the effects of a cantharidine blister.

One bullock, which is still under treatment, is completely covered with crusts from head to foot, but more particularly the head, neck, and behind the elbows; the neck being all in furrows —in my opinion from the animal turning its head about and causing a wrinkled condition of the common integument about this part, at the time the crusts were forming, after the exudation of serum, etc.

As regards treatment, the annexed recipe has seldom been known to fail.

For the Horse.—The affected parts to be well dressed with the following ointment:

℞. Adeps preparatus . . . ℔j.
Sulphur sublimatum . . ℔ss.
Veratrum album . . . . ʒiij.

If the ointment, when made, is of too stiff a consistence, half a pint of train oil is to be added.

If the animal shows any symptoms of the action of the hellebore—which he will do *if too much* is used—these are to be allayed by giving two pints of milk, or one of linseed oil.

While using the ointment, give internally a tablespoonful of the following, by placing it in a small quantity of oats and chaff well moistened, before the animal, once a day.

℞. Potassæ nitras . . . . ʒiv.
Antimonium nigrum . . ʒiv.
Sulphur sublimatum . . ʒiv.

For Neat Cattle.—The affected parts to be well dressed with an ointment composed of

℞. Adeps preparatus . . . ℔j.
Sulphur sublimatum . . ℔ss.,

the veratrum album being left out on account of its irritating effects. Give internally, at the same time, sodæ hyposulphis ʒij., every day in a pint of water.

[In consequence of the number of communications received relating to this disease, we are compelled to reserve our own observations until next month.—Ed. *V. J.*]

TRAUMATIC TETANUS CURED BY NITRITE OF AMYL.

BY W. JOHNSTONE, M.R.C.V.S., MANCHESTER.

ABOUT the beginning of last August, I was called upon by the Manchester Carriage Company (Longsight Branch) to look at a mare which was said to have fallen down whilst working in an omnibus a few days previously. I found both knees badly injured, more especially the near one; the skin was cut directly across, opposite the lower end of the radius, and hung down in a loose flap about three or four inches long, exposing the tendon of the *extensor metacarpi*, which was partially divided, and all the bones of the knee were quite visible. The injury gave every indication of an open joint. I had doubts at the time as to her recovery, but decided to give her a trial. I found it impossible to bring the parts anything like into apposition, the leg being so much swollen. I therefore, by means of a knife, cut the loose flap clean off, and bandaged up the knee with carbolic mixture and tow, and gave orders that it should not be touched until I saw it again, in the course of a day or two. I ordered her to be put into a loose-box, with her head tied to the rack, so that she could not lie down.

I saw her two days afterwards: the wound looked beautifully clean and healthy, with no appearance of synovia, and the mare was feeding and doing well. I continued the carbolic dressings, and had cold water applied to the knees for half an hour morning and night. I visited her occasionally for more than a fortnight; and I never in my experience saw a wound heal so nicely. I now ordered the bandage to be discontinued, the edges of the wound to be cauterized, and the cold water continued as before.

I called again in a few days, and on opening the box door I thought the mare moved rather stiffly; and on applying the twitch (which we had to do in dressing the knees, as she was rather ill-tempered), I saw the *membrana nictitans* protrude slightly over the eye, and her tail stand a little elevated. I said to the man in attendance that I feared she was about to have Tetanus. He said he did not think so: it was only her temper.

I dressed the knees, and did not take any further notice of her, as she began eating. During the afternoon I visited her again, and found my suspicions were confirmed, as she was now undoubtedly suffering from Tetanus. Her jaws were partially closed, with all the usual symptoms of a severe attack. I managed, with some difficulty, to give her a dose of physic, had the box darkened, and told the attendants to keep the mare perfectly quiet.

About twelve months ago, Mr. A. Lawson published in the *Veterinary Journal* a case of Traumatic Tetanus, which he successfully treated with nitrite of amyl; this treatment was afterwards criticised by several members of the profession. I therefore considered I now had an opportunity of proving the efficacy of the drug; so I began using subcutaneous injections of 20-minim doses twice a day: this was on the 16th of August.

The following day all the symptoms were much more marked; the jaws were completely closed, and the mare was utterly unable to eat or drink anything; the pulse was 40, respiration hurried, with perspiration all over the body. I increased the dose of the drug to 25 minims night and morning. She remained much in the same condition for the next five days, the doses now being increased to 35 minims.

On August 23rd she seemed better; the jaws were slightly relaxed, and she appeared to be rather anxious to eat. I gave her some gruel, which she managed to sip a little of. I also had some sloppy bran placed in her manger, and this she had nearly finished next morning. I increased the dose of nitrite of amyl to 40 minims, and she gradually improved; so that in a few days she was able to eat her oats as well as ever she did, although the tetanic spasms had not quite subsided.

I continued the injections (now beginning to decrease the dose) until the end of August, when the mare had perfectly recovered; though the knee had not quite healed.

About a week afterwards I was sent for, as the mare had lain down during the night and torn the wound open, right into the joint. I had her put into slings, and applied the carbolic dressings; but I am sorry to say without effect, as the case became one of very bad open joint. The Company were now

tired of keeping her; and as the knee was certain to be anchylosed should she recover, I advised them to have her destroyed. I think the nitrite of amyl deserves a fair trial, as I am of opinion it is a first-class agent in the treatment of Tetanus.

DOUBLE HERNIA IN A BITCH: DISPLACEMENT OF THE BLADDER SUCCESSFULLY TREATED.

BY J. WOODROFFE HILL, F.R.C.V.S., WOLVERHAMPTON.

ON the 1st of June last, I received into the infirmary a young white bull-terrier bitch, belonging to William Coath, Esq., The Shrubbery, Walsall. The animal in question was sent to me under the supposition she had Mammary Tumours or Cancer, and the following was the brief history furnished me:—Some months previous to the above date, the bitch engaged in a fight with another dog; and to separate the animals, a man had put his foot across the bitch as she lay on her back, whilst he tore her opponent away. At that time she was supposed to be in whelp. Shortly after the combat, these two enlargements made their appearance, and gradually increased in size. On the day she was brought for my inspection, I found them firm, smooth, and glistening, and situated in the neighbourhood of the two posterior mammæ, the latter appearing to be involved. The left side was much the greater, the tumour being as large as a goose-egg and extending to the groin. That on the right was about one-third the size. The bitch being gross, I ordered a dose of castor oil, and light unstimulating diet, until her condition was such as to warrant an operation being performed if necessary.

On frequent examinations made between the 1st and 21st, I found the tumours varied somewhat in their condition: at one time being softer and lower in temperature, at another the reverse. The presence of fluid was always evident. On the latter date I made a most careful examination, the patient being then considerably reduced in flesh. The left tumour was

at this time exceedingly tense, hot, and larger than it had ever been; moreover, the bitch evinced pain, especially on manipulating it, and had not been observed to micturate with her usual frequency. I at once determined to explore this particular tumour with a fine trocar. This I inserted at the right of the nipple, and when withdrawn there came through the canula a full stream of hot urine—altogether nearly a breakfast-cupful. With this evacuation the enlargement entirely disappeared, and nothing remained but the mammary gland. The other tumour also became less—owing, doubtless, to removal of the pressure consequent on the distended bladder. All doubt now as to the nature of the tumours was at an end. It was clearly a case of double rupture; the sac tapped contained the greater portion of the bladder, with probably a small knuckle of intestine, while the opposite tumour was entirely intestinal. The peritoneal rupture on either side—which it was previously, notwithstanding careful manipulation, owing to the size and fulness of the enlargements, impossible to detect—could now be distinctly felt.

The following morning the same state of affairs existed, and again I passed the trocar with a similar result, repeating the operation for two or three days. In the meantime, she was prepared for an operation, which I performed on the 28th; Mr. C. A. Newnham, surgeon, who kindly assisted me, being present, also his son, and several gentlemen who took an interest in the case.

The bitch being put under the influence of chloroform and placed on her back, and the hind parts lifted a little off the table to facilitate the reduction of the hernia, I made a longitudinal incision to the right of the nipple. On dividing the sac, intestine alone was visible, and the absence of bladder was accounted for by its having been emptied before the operation; otherwise it would have occupied the entire sac.

Having reduced the entire hernia, I closed the peritoneal rent, which was a little less than two inches, with uninterrupted catgut suture, and the external wound with interrupted silk sutures. A light wire muzzle was then put on the patient, to prevent her gnawing or licking the parts, and she was made as comfortable as possible.

The diet for the first fortnight was milk-and-water, sweetened;

subsequently pure milk; and, finally, bread and milk, until the part was well. The case, I am pleased to say, went on as satisfactorily as I could have wished. The last external suture came away on the twenty-third day, and within four weeks all was perfectly healed, and no enlargement apparent, or anything to denote what had been amiss beyond a fine seam where the incision had been made.

Having allowed the patient a month to recover condition, and establish the firmness of the part to be established, I proceeded to prepare her for the second operation—viz., the reduction of the hernia on the opposite side. This took place on the 11th of August, in the presence of Messrs. Newnham and Manley, surgeons, and others. The operation was conducted under chloroform, in precisely the same manner as on the first occasion. I should observe that this rupture, which in the first instance was comparatively small, had increased considerably in size, and was now larger than the one already operated on. This I attributed to the pressure brought to bear on it from reducing the opposite large one, which must necessarily have had a tendency to force the smaller one further out. As may be imagined, I did not feel so sanguine about the result of the second operation as the first; for I had grave doubts whether, in reducing this *increased* rupture, the opposite one would not break through again. However, as just mentioned, I operated. The hernia was intestinal; the peritoneal rent was not so long as in the other, but the hernial sac was considerably thicker and more numerous in its layers. On the 25th, exactly a fortnight afterwards, the last stitch was removed; and at the time I am writing these notes—the 10th of September—the bitch is perfectly well, in good condition, and in excellent spirits.

The diet was the same as that prescribed after the first operation. Probably it would have been considered more scientific had I pared the edges of the ruptured peritoneum; but I preferred—as I do in similar cases in other animals—trusting to lymph effusion and its subsequent organization, rather than incurring the risk of producing general and acute Peritonitis, which paring an old ruptured peritoneum would tend to do.

It is perhaps scarcely necessary to point to the obvious cause of this double rupture—viz., that the peritoneum was broken in the act of tearing away the other dog, while the man's foot across the bitch's abdomen held her firmly down—a cruel, but we trust an unintentional procedure.

THE TRANSMISSIBILITY OF CONTAGIOUS PLEURO-PNEUMONIA TO OTHER ANIMALS THAN THE OX TRIBE.

It is somewhat strange that we are but little acquainted with the transmissible powers of the destructive pulmonary disease known as Contagious Pleuro-pneumonia, and that veterinary writers all appear to agree that it is strictly limited to cattle—*i.e.*, *Bos taurus*. True, Dr. Woodward, in his excellent description of the pathological anatomy of the disease (Report of the United States Commission of Agriculture for 1871), and which Dr. Yeo, in the very imperfect bibliographical notice prefixed to his report (published in the *Veterinary Journal* for last July and August), has overlooked, states that he had dissected a tame deer—*Cervus Virginianus*—which had died of the malady, and found the lungs in the same condition as those of diseased cattle. This instance of the extension of the malady to the cervine tribe, if there has been no mistake, is interesting; but we believe it to be unique, and therefore there is reason to doubt its reliability. If the malady could be transmitted to the *cervina*, surely we must have had abundant proof offered us in those countries in which the affection prevails, and where deer and cattle come in contact. But, even with regard to other species of the *Bovidæ* than the common ox, we have hitherto had no evidence of susceptibility, notwithstanding their closer relationship to the latter. We are not aware that sheep have ever been infected, notwithstanding their intimate association with cattle at pasture. Spinola certainly asserts that he has seen goats affected, but he stands alone among veterinary authorities in this observation.

From a recent report of the German Sanitary Board (*Veröffentl. d. Kaiserl. Deutschen Gesund.-Amtes*), we derive the interesting information that in the Brussels Zoological Gardens, in 1877, there was an outbreak of Contagious Pleuro-pneumonia, and that two yaks (*Bos* or *Poephagus grunniens*), three bisons, and one buffalo were victims; the last-named animal was killed, the others died. A careful examination of the animals was not possible during life, as no one dared venture in their shed; an autopsy of the six, however, left no doubt as to the nature of the disease with which they had been affected. The pathological alterations in the lungs were most marked, and very characteristic of Lung-plague (*Lungenseuche*); the various stages of hepatization in these organs were even more distinctly defined than in cattle, and the interlobular connective tissue contained a very abundant exudate. In the yaks and bisons, the course of the disease was rapid.

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Editorial.

THE TITLE OF VETERINARY SURGEON.

THE most pressing question which now lies before the veterinary profession in this country, and to the solution of which we trust it will soon address itself in earnest, is that relating to the best manner in which it can protect its title from misappropriation, and thus protect itself and the public from the mischievous and oftentimes injurious effects of imposition.

As is well known, since its commencement this Journal has stood alone in urging the need there exists for some means of restraining unqualified persons from illegally assuming the title of Veterinary Surgeon; and we have on every opportunity drawn attention to the discouragement, and something worse, which practitioners were compelled to submit to, through having to compete with, and be placed on the same level as, men who could in no respect claim to be veterinary surgeons or represent the veterinary profession. Our views and arguments have certainly been opposed by those of others, who, we cannot deny, had the interests of the Body Corporate quite as much at heart as ourselves; but we have repeatedly ventured to point out that they did not look at the matter in the same light as those who have carefully studied it, and have had some practical experience in it. Their chief argument has been that a good practitioner did not require any protection from quacks who designated themselves "Veterinary Surgeons," or even "Members of the Royal College of Veterinary Surgeons," and that the public would be certain to find out who was the one most deserving of patronage. Consequently, there was no need for interference, and matters should be allowed to go on as they have been doing.

Now, to put the most charitable construction upon it, this argument is a very narrow-minded and superficial one. In the first place, it is not always the best practitioner (*i.e.*, curer of disease), either in human or animal medicine, who obtains the largest amount of patronage; for we know that other qualifications and attributes, beyond a good knowledge of medicine and surgery, are essential to success. And the non-qualified man, having no kind of professional check to restrain him, can stoop to measures and resort to devices which a practitioner, responsible to his profession and the public, and aspiring to do his duty faithfully and honestly, cannot, dare not do. Besides, the public cannot always judge as to the merits of qualified and non-qualified men, nor do they often care to inquire if every one who designates himself a Veterinary Surgeon has really any claim to the title. From the increasing value attached to the services of members of our profession, and the confidence which is beginning to be placed in their skill, few indeed except they are now-a-days entrusted with the care of sick animals or requested to give advice; and when a veterinary surgeon is employed, there can be no doubt that ninety-nine per cent. of the public

believe they are obtaining the services of a gentleman who is a graduate of the Royal College. It would seldom, if ever, enter the mind of those who need professional assistance, to inquire whether there are men so designated who are not qualified, and have no right whatever to assume the title. They accept the man, if he be non-qualified, as any ordinary person would accept a counterfeit coin or an adulterated article of food, and are compelled to put up with the result—the groom, coachman, or cowman, if he chance to know the difference, generally having a pecuniary or other interest in recommending such a person. Thus it is that the public are cruelly imposed upon, and much torture oftentimes inflicted on their animals, without any chance of discovering the imposition or obtaining redress, should they happen to be enlightened when too late. In this way the owners of animals, as well as the animals themselves, are made to suffer, and obviously require protection. At any rate, the public should be placed in a position in which it may know whom it is employing—a qualified or a non-qualified man. In no other profession is the assumption of a title calculated to mislead and injure the public interests allowed. Surely in ours, in which so much animal wealth and health is concerned, and animal suffering so greatly at stake, the law should intervene and prevent fraud, loss, and suffering.

When our first charter set forth that none but those who had studied veterinary medicine at recognised veterinary medical schools, and had successfully undergone the necessary tests as to proficiency, were to be designated Veterinary Surgeons, it was certainly intended that the public should be so protected, as well as the profession. That this protection has not been long ago secured, is more due to the lamentable condition of the profession for so many years, than to any other cause. There has been greatly too much school, and far too little Royal College influence exercised.

An idea seems to prevail in some minds that we are agitating in favour of a movement having for its object the suppression of quackery. We are doing nothing of the kind, and we are not so Quixotic as to imagine for a moment that quackery can be directly combated. By asking Parliament for a short Act which will enable us to prevent any but recognised graduates from designating themselves Veterinary Surgeons, we are in no way interfering with the occupation of the pretender. We are only desirous, firstly, that the public may know whom they are employing, and, secondly, that we may have preserved to us our own title, upon which our good name and progress so much depend. We do not seek to interfere with the vocation or the business prospects of the horse-doctor, cow-leech, or "canine surgeon," but beg that they may be known by these or any other designations they choose to adopt. We have not a word to say against these men, many of whom are clever, respectable, and useful in their sphere of life, nor have we the slightest desire to diminish their practice; but there is no reason why they should be allowed to represent themselves to be what they are not, or to pursue their calling under any false title or pretence. They are welcome to compete with veterinary surgeons if they can find clients;

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and the owner of a sick or unsound animal ought to be at liberty to consult whom he chooses. Only he should know the real status or qualifications of the man whom he must depend upon for important services, and not be left to the risks of false representation and imposition.

The Body Corporate is responsible for the professional competency of its members, and the public will not exempt it from this responsibility. It is also, to a certain extent, responsible for the moral conduct of its graduates; and as a proof that it is so, and that it does not wish to evade its obligations, in the Supplementary Charter recently obtained it has acquired the power of removing from its Register the name of any veterinary surgeon whom it may consider has misconducted himself in such a manner and to such an extent as to merit that severe punishment.

Over those who usurp the title of Veterinary Surgeon there is no check or control whatever, and the public have no guarantee for their moral or professional behaviour. Neither can the profession protect itself from the stigma which attaches to their *lâches* and misconduct, but must submit to the degradation of having their sins of omission and commission laid to its charge; as they wear the assumed title, and the public cannot distinguish the false from the true, the counterfeit from the genuine. We have not to wait long or go far for evidence of this, and almost weekly we are confronted with a notice like the following, which we extract from the *Birmingham Daily Gazette* of August 30th, but which also appeared in many of the London and provincial newspapers of about the same date:—

"A VETERINARY SURGEON FINED £20.—At the Nuneaton Petty Sessions yesterday, Choyce Chapman, veterinary surgeon of that place, was charged with assaulting and beating his wife, Mary Ann Chapman, at Nuneaton, on the 12th inst. He came home drunk, dragged his wife out of bed, tried to throw her downstairs, and threatened her life. The magistrates committed the defendant to prison for three months, with hard labour. Mrs. Chapman, the wife of defendant (through her solicitor, Mr. Bland), expressed a hope that the Bench would reconsider their decision, as sending the defendant to prison might prove ruinous to him. After consideration, the magistrates decided to accede to the application. They gave the defendant the option of paying a fine of £20, and costs £3 11*s.* 6*d.*, but in default of payment of the whole amount in a week, the law would take its course, and defendant be committed to prison for three months, with hard labour. The Bench also decided that the wife should no longer be bound to cohabit with her husband, but that she should have the custody of the two youngest children, and that defendant should allow her 15*s.* per week on which to support herself and them."

Now, this person is not a veterinary surgeon; his name does not appear in the Register, and he has no right whatever to the title. Yet it will be perceived that the profession has to bear the shame and the odium attaching to his offence. Surely this is most unfair to us, to use the mildest term possible; and surely those among us who have opposed the proposal that a penal clause should be obtained to remedy this damaging state of affairs, have no desire to claim such men as colleagues and associates!

In direct proportion as the study of veterinary medicine becomes more thorough and exacting, and as the tests of proficiency are more

severely and searchingly applied, so, in all probability, will the number of those who illegally adopt the title of Veterinary Surgeon increase.

It is therefore incumbent upon those of us who hold the title in respect, and value it as a passport of education and competency, as well as a guarantee to the public that its possessor is bound to do his duty and act honourably, to take early and energetic steps to obtain what has been so urgently needed for many years—an Act which will protect the public and the profession. To obtain the diploma of the Royal College requires much money, and long and close study. It is most unjust that its possessor should find himself on the same level with the ignorant pretender and impostor, and compelled to bear a share of the ignominy or disgrace which the latter incurs so frequently.

Now that the Highland and Agricultural Society of Scotland has consented to abandon its certificate-dispensing, the only barrier in the way of putting a stop to the false assumption of our title is removed; and it will certainly be the fault of the profession itself, if another session of Parliament passes without a prohibitory Act being obtained. All we require is power to summon any offender before a magistrate, who may, on conviction, inflict summary punishment. The effect of such an Act would be wonderful in many respects, but particularly with regard to sign-boards, door-plates, and bill-heads.

NEW DRUGS, ETC.

The Therapeutic Value of "Soot."

In all our modern works treating on Zoötherapeutics and Zoöhygienics, no mention is made of that common domestic nuisance "Soot." Yet we are informed by "F," a correspondent in our widely circulating contemporary *The Field*, of the remarkable value of this "agent" as a therapeutic in *Spasmodic Colic*, and that, too, after the "exhibit" of linseed oil, opium and turpentine injections, and other "severer measures," had entirely failed to relieve the poor suffering creature, "the patient." Messrs. Day, Son, Hewett, and Company's gaseous and other fluids are all doomed now. Mr. Chimney Sweep, and not Mr. Pharmaceutical Chemist, will be sought after by all agriculturists, country gentlemen, and others residing at a distance from the qualified veterinary surgeon; and even those residing next door, who may be economists, will be likely to patronise their regular "sweep" rather than their regular veterinary surgeon.

"Spasmodic Colic.—Seeing reference to the above in the last *Field*, perhaps you would not object to a little more on the same subject, and would insert the following. A short time since a neighbouring farmer sent word to me that one of his horses was very sick, and asked if I would go to see him. It is pretty generally known in the neighbourhood that I usually keep a few of the medicines suited for the horse, and, besides, am armed with a few instruments useful in some cases—such as a clyster pipe, drenching horn, fleams, etc. Accordingly, in compliance with the summons, and armed with the above instruments, I went to the farmer's house. I at once saw

that the horse was suffering from a very severe attack indeed of Spasmodic Colic. Accordingly, I proceeded to 'exhibit,' as the technical phrase goes, the medicines suitable on such occasions, among which the late Mr. Olden, of Cork, told me the following are about the best: a mixture of linseed oil, opium, and turpentine. I also used the syringe freely, and threw up a good large quantity of liquid commonly used in injections. These seemed to have no effect, so I resorted to severer measures, according to directions from the same source. But all seemed of no avail, and the horse was apparently becoming worse. An old man who was in the employment of the farmer at length said, 'Well, sir, you have tried your remedies: will you now allow me to try mine?' 'You may try anything you like,' I said; 'for I do not think the horse has an hour to live, and I would not give one penny for his chance.' 'Well, sir,' said the man, 'an old man told me that nothing was so good for that kind as common soot: mix up a good bowl of it in some water, and drench the horse with it.' 'Try it by all means,' said I, 'and there is the drenching horn for you.' As it was late I returned home, expecting to hear next day that the horse was dead, and that I should send for him for the Cashelmore hounds; but, on the contrary, I heard that from the time he got the soot the violent pain abated, and by degrees he became perfectly well, and on the second day was able to resume his work on the farm, where he is now. It may be very well and right for an owner to send for a veterinary surgeon when disease occurs; but in this locality the nearest vet. is twenty miles distant, and in some cases—that of Spasmodic Colic among them—the fate of the horse would probably be sealed one way or other before his assistance could be had.—F. (August, 1878.)"

The dose was a "bowl of soot." Now, we are not informed whether the said bowl was a kitchen, washstand, or punch bowl. Doubtless this small matter is immaterial: a bowl, and the "exhibit," after all the former "exhibits" and *severer measures* had failed, positively cured the patient labouring under an attack of Spasmodic Colic, from which there was no hope of recovery; indeed, "F" states the patient had but *an hour* to live. Judge his surprise and astonishment the next morning, and the disappointment at the kennels of Cashelmore!

Our surprise is equal: not because he had recovered an attack of "Spasmodic Colic"—for patients generally do recover, it is very rarely they succumb to it—but that the patient pulled through the "exhibits" of such horrid nostrums; and then, when kind Nature, despite all counteractive treatment, had accomplished her task and restored health, it is ascribed to the potent agency of a "bowl of soot."

To be serious, it is to be hoped the owners of animals will not be led to resort to this new "therapeutic" or domestic nuisance. They have too much of that kind of sense styled "common," and will furnish themselves with a few effective mixtures from their nearest qualified man. Then, when a horse goes amiss, they can give both patient and veterinary surgeon a chance, by stating what has been done. The veterinary surgeon then would know of what the "exhibit" was composed.

We have heard too much of "horrid nostrums," *vel* "exhibits," during twenty-five years' practice in wide agricultural districts, and trust, with the progress of enlightenment, soon to know that the cause and prevention of disease will be advocated, rather than *new* "exhibits," and particularly by gentlemen amateurs in the veterinary art.

NORMAL AND PATHOLOGICAL PNEUMOGRAPHY.

MM. LAUGERON AND LAULANIE, Professors at the Toulouse Veterinary School, offer some extracts from their thesis on the above subject for the diploma of Veterinarian. The object of this thesis was to show what clinical assistance may be obtained from the application of the graphic method, when applied to the study of the horse's respiration in diseases of the chest. In order to arrive more certainly at satisfactory results in this direction, they first studied normal, and subsequently abnormal, pneumography in this animal. Though they are not perfectly satisfied with the result of their attempts, as they were unable to determine, with all the necessary precision, everything pertaining to morbid respiration, owing to the shortness of time and the want of a sufficient number of varied cases; yet they are confident that the more salient facts they have elucidated, and particularly those pertaining to the relations between the abdominal and thoracic respiration, as well as to the characteristics of "Broken Wind" (*Pousse*), will prove interesting. In this confidence we fully share, and we have much pleasure in giving an account of their investigations, with the illustrative diagrams, the wood blocks of which have been kindly furnished us by Professor Bouley, Inspector of the Veterinary Schools of France, and editor of the *Recueil.*

The instruments employed in their researches were the pneumographs invented by Marey, the oscillations of which were noted by means of lever drum and the Universal Registrar. Their thesis is divided into two parts. The first treats of normal respiration, with regard (1) to the thoracic movements, and (2) the abdominal movements and their relation to those of the thorax. In the second part, which is devoted to morbid respiration, there are studied: (1) Acute and Chronic Pneumonia, Acute and Chronic Pleuritis or Hydrothorax, and (2) Broken Wind.

NORMAL PNEUMOGRAPHY.

The Thoracic Movements in the Horse.

The numerous tracings of normal respiration obtained by the experimenters led them, with regard to the *thoracic movements,* to conclusions analogous to the propositions established by M. Rodet, the learned Director of the Lyons Veterinary School, in his communication to the Medical Society of Lyons in 1868, which are formulated as follows:—

1. Inspiration is accomplished in a uniform manner, except in some cases in which, towards the finish, there is a slight protraction or slackening, in such a fashion that the line representing this movement, instead of being sensibly straight, is slightly curved or broken.

2. In a general way, expiration, like inspiration, is made at one time,—*i.e.,* in this second part of the respiratory movement the costal parietes have a uniform movement, represented in the tracing by a straight line, oblique on the line of abscisses. It is only exceptional that the commencement of expiration is a little more rapid than its termination, but it is not to be compared with the initial period of expiration in an adult man.

3. There is no pause in normal respiration, the thoracic movements succeeding each other without interruption.

4. The duration of inspiration relatively to that of expiration is most frequently found to be, as indicated by Rodet, : : 16 : 21. But numerous variations are discovered, and it even sometimes happens that inspiration is shorter than expiration.

The Abdominal Movements, and their Relations to the Thoracic Movements.

The collected traces of the abdominal movements in the horse during

inspiration, may be grouped around the two types represented in Fig. 7, but it is mentioned that those like A are most numerous.* The latter differ from the thoracic traces, in so far as they indicate, towards the end of expiration and commencement of inspiration, movements entirely opposed to those of the pectoral walls; while those analogous to the tracing B resemble in every respect the diagrams of thoracic respiration.

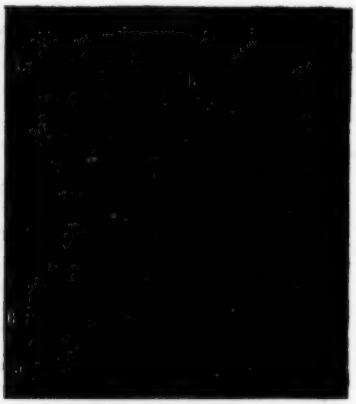


FIG 7.—ABDOMINAL TRACINGS. NORMAL RESPIRATION.

These differences at first caused the authors much trouble, but they soon remarked that the animals which gave tracings like A had the abdomen very developed—a conformation which caused the girdle of the pneumograph to shift towards the pubis; while those which furnished tracings like B had the belly little developed, so that the pneumograph could be maintained in a more forward region. This observation renders more marked the characters offered by the abdominal tracings of the horses suffering from Pneumonia, whose belly became "tucked-up" under the influence of the diet. This coincidence, however, not always being sufficient to account for it, recourse was had to the following experiment :—

A Poitevine mare, which was "cow-bellied," gave a tracing like A, the pneumograph occupying the posterior abdominal region; then the apparatus having been held by assistants in front of the umbilicus, a tracing was procured similar to B.

The preceding results seem to show that the peculiarities in the abdominal tracings are due to the displacement of the digestive viscera. In order to ascertain what agencies presided over these displacements, and to determine exactly the influence of the thoracic movements on the play of the abdominal walls, the phrenic nerves of a horse were divided, after tracings of the thoracic respiration and the abdominal movements had been obtained. When the animal had recovered from the immediate effects of the opera-

* In all the tracings, inspiration is marked by a descending, and expiration by an ascending line. Thus, in B the expiration commences at *a* and stops at *b*; while the inspiration begins at *b* and finishes at *c*. It is the same in A, but the point *b*, corresponding to the commencement of inspiration in the thoracic parietes, belongs also to the expiration of the abdominal walls.

tion, in about half an hour, tracings were again taken from the chest and abdomen. These latter, compared with those obtained before the vivisection, indicate the results following paralysis of the diaphragm, and their analysis is interesting. The tracings obtained before the division of the nerves were those of normal respiration. In Fig. 8 are given those taken afterwards, A being the thoracic and B the abdominal tracing.

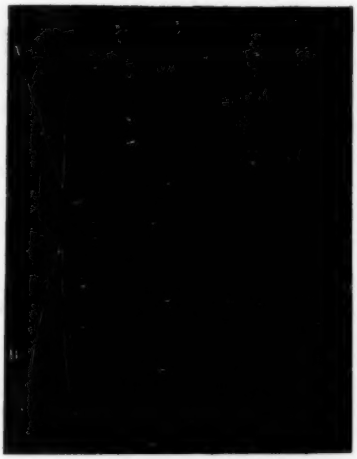


FIG. 8.—AFTER SECTION OF THE PHRENIC NERVES.

The tracing A differs from the typical tracing of normal thoracic respiration in the expiration being accomplished in two movements or times, the first of which is exceedingly abrupt, and by two slight checks in inspiration. The abdominal tracing B is still more regular, and offers two sharp falls, one corresponding to the commencement of inspiration, the other to the beginning of expiration, between these being two gradual ascensions. The result, in fine, appears to indicate that during a complete respiratory movement two abdominal expirations are produced.

It is easy enough to interpret this succession of movements in the abdominal walls. Inspiration begins at the same period in the thoracic and abdominal parietes, it being in the latter merely the result of relaxation of the abdominal muscles. But the diaphragm being paralysed, thoracic inspiration should be entirely effected by the elevation of the ribs: this elevation attaining extreme proportions, especially in the hypochondriac regions, and the digestive viscera being drawn forwards to occupy the vacant space. The volume of the abdomen should, therefore, diminish posteriorly, its parietes becoming elevated and executing a movement analogous to that which they accomplish in normal expiration; but one point must be insisted on,—*i.e.*, that in this movement the abdominal muscles are entirely passive, and having nothing to do with expiration but the form. Such is the interpretation given to the ascension which, in the tracing B, corresponds to thoracic inspiration.

With regard to the second abdominal descent, synchronous with the commencement of costal expiration, this is due to the sudden driving back

towards the pelvic cavity of the digestive apparatus, energetically compressed by the precipitous approximation of the hypochondriæ. This primary effect being produced, thoracic expiration is carried on slowly, and the contraction of the abdominal muscles may be efficaciously exerted on the digestive viscera, which are gradually pushed into the position from which they had been expelled. In this way there occurs, through the contraction of the abdomen in its posterior part, that ascension which, in the tracing B, is produced during the second part of thoracic expiration—an ascension which represents an active expiration of the abdominal walls.

The results of this experiment are very curious, and demonstrate that the position of the digestive viscera is capable of being greatly modified by the manner in which the mechanical phenomena of respiration are accomplished. There will be found, in treating of "Broken Wind" in the third period, a new confirmation of this proposition. This explanation is also invoked in the case of the special form of the abdominal tracings in normal respiration: the graduated fall which succeeds the commencement of expiration ought to be due to the progressive repulsion towards the pelvis of the intestinal mass by the slow approximation of the hypochondriæ; the other details being accounted for by the predominance in action of the diaphragm or the external inspiratory muscles.

Pathological Pneumography.

Acute and Chronic Pneumonia. Acute and Chronic Pleurisy or Hydrothorax.

Acute Pneumonia.—The mechanical phenomena of the respiration in animals affected with Acute Pneumonia were studied by Laugeron and Laulanie, and tracings taken at all periods of the disease, but nothing could be gained from these which would give an exact appreciation of the gravity of the lesion. Thus, in horses in which Pneumonia of a gangrenous character had reached its last stage, diagrams were obtained that only differed from the tracings of normal respiration by greater frequency and a less amplitude of the movements; while in other patients, which afterwards recovered, and were only in the early stages of the disease when the tracings were taken, the respiratory curves were very irregular. Otherwise, there was no absolute distinction for any of the phases of the malady.

Chronic Pneumonia.—Two cases of Chronic Pneumonia were studied: one of which, being destined for dissection, enabled its condition to be verified by the autopsy. The tracings registered in these conditions are very curious, in so far as they indicate the "check" or "pause intervals" (*temps d'arrêt*) of the jerks or starts in the thoracic walls; though, if they offer much interest in themselves, they are nevertheless not characteristic of the nature and extent of the pulmonary alterations, as they scarcely differ from the tracings of "Broken Wind." The value of the graphic system, when applied to the diagnosis of Chronic Pneumonia, is therefore slight, owing to the respiration being interrupted or jerking. The tracings of "Broken Wind" being given hereafter, the authors do not dwell any further on this *vieille courbature.*

Acute Pleurisy.—The diagrams of the thoracic respiration in Acute Pleurisy in the horse are characteristic of the affection by the frequency, the trifling amplitude, and the irregularity in the movements which they display (see Fig. 9). But direct observation of the play of the pectoral walls is so easy in this case, that the malady may be readily recognised by the ordinary means of investigation. A general observation is made with regard to this subject: it is that, as might be predicated, the employment of registering apparatus in these maladies only yields information which is often insufficient, and little different from that which may be obtained by direct examination

of the respiratory movements. These movements, by their amplitude and the slowness in their succession, are easy to note; but it is not so when they are convulsive, as in "Broken Wind"; and when that condition is alluded to

FIG. 9.—ACUTE PLEURISY.

the advantages to be derived from the graphic system in arriving at an exact determination of the respiratory movements, which are far removed from the physiological type, will be obvious.

CHRONIC PLEURISY OR HYDROTHORAX.—A certain number of tracings relating to this affection were collected. They all resemble each other with some slight exceptions, and demonstrate that the principal alteration in the rhythm is related rather to the number than the form of the respiratory movements. The authors were inclined to believe, *a priori*, that the fluid contained in great quantity in the pleural cavities ought, by a physical action, to modify the form of the tracings. But this was not so; for there was nothing in these which would have led any one to suppose the existence of so considerable a lesion as the effusion of fluid into the chest of the animals which furnished them.

This point would not have been insisted upon, had there not been observed, *apropos* of Hydrothorax, a symptom which ought to be looked upon as pathognomonic, and which had not been noticed before. This is the *discordance of the flank* indicated by M. Saint-Cyr, in Bouley and Reynal's *Dictionnaire*. The learned Lyons professor writes: "Soon the flank presents that remarkable alteration which we designate by the name of *discordance* in the respiratory movements. Then, at the moment when the

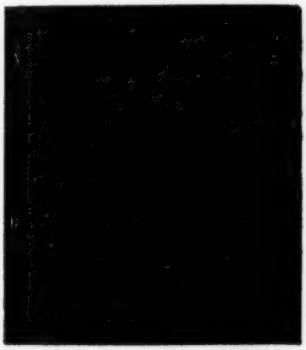


FIG. 10.—TRACINGS OF HYDROTHORAX.

ribs are being elevated for inspiration, the flank is hollow; but when the ribs fall the flank is filled by the projection backwards of the abdominal viscera."

The authors have closely studied the rhythm of the respiratory movements in horses whose thoracic organs were perfectly healthy; and, what is remarkable, if they had to describe them they assert that they could not do better than borrow the exact words employed by Saint-Cyr with regard to Hydrothorax. In this condition, in fact, the form of the movements is not altered. If the tracings are consulted (see Fig. 10), it will be seen that the thoracic and abdominal movements are allied by the most perfect synchronism.

With regard to the exact interpretation of the signs furnished by direct examination, the following are given: In inspiration, the mass of viscera is thrown backwards by the diaphragm, and its weight carries the floor of the abdomen below the line of the flank. Under the influence of this weight, a vacuum is made in the higher region of the flank, and the atmospheric pressure depresses the hollow of the flank. In expiration, the abdominal muscles, by their contraction, displace the viscera, which are thrown forward, and a large portion of them is lodged in the concavity of the diaphragm. The tendency to a vacuum diminishes in the lumbar region, and the flank is elevated. How could it be otherwise? It is certain that all the abdominal muscles are synergetic in their expiratory action; the small oblique, which forms the base of the flank, therefore contracts during expiration, and in contracting it becomes tense. This interpretation is not in opposition to what has been already said as to the abdominal movements, with reference to the tracings of the abdomen during normal inspiration. It is therefore concluded that respiration in Hydrothorax is not modified in its general form, and that the sign described as *discordance of the flank* is a normal phenomenon. The authors point out that they are not alone in this view of the matter. In the *Dictionnaire de Méd. Vétérinaire*, etc., of Bouley and Reynal, in the article "Flank," an interpretation is given of the movements of the abdominal walls analogous to the above. M. Bouley, who writes the article, says: "The following are the phenomena manifest in the region of the flank in the physiological condition. At the moment of inspiration the abdomen enlarges and descends; the circle of the hypochondrium is effaced; the small oblique muscle is pushed, as it were, outwards in the lower part of the flank, while it is markedly depressed and hollowed at its upper portion—a physical fact resulting from the atmospheric pressure, the action of which is greater in the upper region of the flank as the increasing amplitude of the abdominal cavity offers a larger space to the organs it contains, and allows them to descend to a lower level; whence the greater depressibility of the roof of this cavity. During expiration the succession of phenomena is reversed. The abdomen contracts and ascends; the circle of the hypochondrium, which it previously exceeded, is now very prominent. The flank contracts at its inferior part, and from the ilium to the ribs, between which is expanded the small oblique muscle, is seen a kind of thick cord passing obliquely downward and forward, and which is nothing more than the central portion of that muscle, very tense and standing out in relief between these two points of attachments. Above this cord, the hollow produced during inspiration become less marked."

"Broken Wind."

Under the designation of "Broken Wind" (*pousse*), Laugeron and Laulanie studied that variety of morbid disturbance in the horse's respiration belonging to this affection, or rather group of affections. They adopt this designation in preference to that of *Pulmonary Emphysema*, as they did not always pre-occupy themselves with an exact diagnosis of the malady, and rarely had an opportunity of making an autopsy of the animals they experimented upon. They were always able, however, to assure themselves of the

existence of "Broken Wind." They remark that the diagnostic signs derived from auscultation and percussion of the chest, although of very great assistance in determining disease in the thoracic cavity, cannot always establish, in a formal manner, the nature and seat of the lesions,—at least in the domestic animals. The indispensable *criterium* of these kinds of study is the autopsy; but this, unfortunately, can only be exceptionally practised. The diagnosis of "Broken Wind" is easiest made, and is reduced to detecting a symptom.

A large number of tracings of "Broken Wind" were taken, and these offered a great variety, which was generally related to the period which the affection had arrived at. But they could be divided into three types.

FIRST TYPE.—"BROKEN WIND" AT THE FIRST PERIOD.—In Fig. 11 the thoracic tracing of a horse affected with "Broken Wind" in the first stage is given. As is readily seen, the respiratory movements are as yet

FIG. 11.— BROKEN WIND" AT THE FIRST PERIOD.

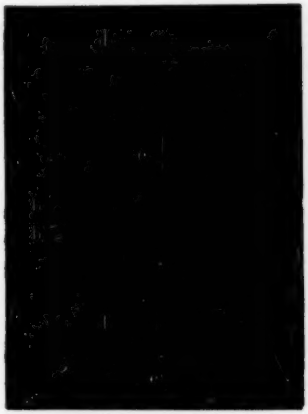


FIG. 12.—"BROKEN WIND" AT THE THIRD PERIOD.

but little disturbed, and there is only a slight check in inspiration, indicated by the horizontal deviation at *a*, which separates each line of the respiratory movement into two parts.

SECOND TYPE.—"BROKEN WIND" AT THE THIRD PERIOD.—The second type is represented by the tracing A in Fig. 12. B is an abdominal tracing, taken simultaneously with A, both corresponding to "Broken Wind" at the third period; they are only shown here in order to afford a more exact and easy appreciation of those belonging to the affection at its second period.

These diagrams are very curious, and exhibit details in the mechanical phenomena of respiration until now unknown, so far as the authors could discover. In order to give a more exact account of these peculiarities, the thoracic and abdominal tracings are compared, their analysis commencing with the initial expiratory action of the abdominal parietes.

The contraction of the abdominal muscles is exceedingly abrupt and energetic, and rapidly produces a very great diminution in volume of the abdomen; the pen of the lever-drum of the graphic apparatus describing

in the tracing B an almost vertical and very extensive ascending line *a' b'*. At the same time there is inscribed in the tracing A the broken line *a b*, to which the following signification is attached: expiration commences at the same time in the thoracic and abdominal parietes, and this is indicated by the slight ascent of the pen from *a*; but the falling of the hypochondrium cannot continue, hindered as it is by the considerable pressure exercised on it within and without, from the carrying forward of the digestive viscera suddenly compressed by the energetic contraction of the abdominal muscles. Far from being depressed, the hyprochondrium is itself raised, and to such a degree that to the slight ascent of the pen succeeds a sudden fall—the descending portion of the line *a b*. From this is deduced the remarkable fact that expiration has commenced in the lungs, even while the ribs are continuing to be elevated—at least, in the posterior part of the thorax. But the inspiration was already deep, the ribs being in a manner twisted on themselves to accomplish it; the new and passive elevation, which results from the pushing of the abdominal viscera, submitted to strong pressure, therefore again exaggerates the elasticity—a consequence of the deformity of the costal arches. Also, when this internal pushing diminishes in intensity, a lively reaction occurs in the thoracic parietes, the subsidence of which is rapid. Under all circumstances, the descent of the ribs does not take place at one time, but occurs at twice, the movements being divided by a slight check or a simple retardation; finally, the movement becomes gradually less rapid, and is even soon followed by a pause, or absolute rest, which precedes the commencement of inspiration in the succeeding respiration.

To these oscillations of the thoracic parietes there are corresponding modifications of an inverse order in the abdominal walls, as is seen in tracing B. As soon as the ribs begin to fall quickly, the abdomen commences to increase in volume, and the pen draws a rapidly descending line which, like the expiration line of the thorax, is divided into two parts separated by a horizontal portion marking a halt or check; then the abdomen enlarges again, but slowly, or its dimensions remain sensibly stationary—phenomena indicated in the tracing B by the slightly descending or even horizontal line placed after the point *c*. This discordance in the action of the abdominal and thoracic walls is explained in the same way as that which occurs at the commencement of abdominal respiration—by the displacement of the movable viscera contained in the peritoneal cavity. The sudden approximation of the hypochondriæ should necessarily cause an instantaneous and energetic pressure on the digestive organs of the same kind as that exercised by the abdominal muscles, and consequently a displacement analogous to what takes place in the latter case, though in an inverse direction. The termination of costal expiration, indicated by a slow movement and a relatively long pause, leaves the intestinal mass occupying the position it has assumed under the influence of the sudden drawing together of the ribs; and thus the abdominal walls are feebly elevated or remain immovable.

Inspiration commences rapidly, but is suddenly interrupted, to recommence with almost equal rapidity after a certain period of rest. These characters are portrayed in tracing A by the line *d e*. The portion *d' e'* in the tracing B, which corresponds to *d e*, shows that the abdomen undergoes variations in volume which are capable of being ascribed to the same causes as those already referred to; for it is conceived that the separation of the hypochondriæ is rapidly produced, and in a relatively extensive proportion, exercising in the anterior part of the peritoneal cavity a kind of aspiration which causes the displacement of the intestinal mass in that direction; whence the diminution in size of the posterior region of the abdomen.

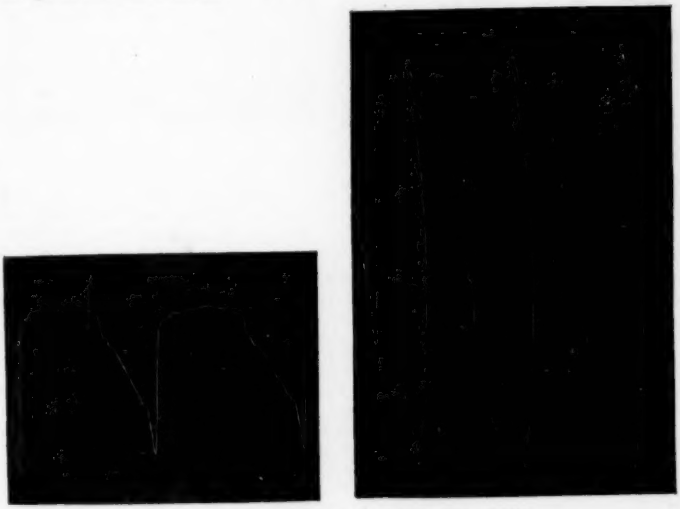
But this state of affairs cannot persist; the diaphragm loses its convexity

by contracting, and throws back the digestive viscera towards the pelvic cavity.

It is therefore considered that inspiration commences at the point *d*, that it terminates at *e*, and that it is *interrupted;* that expiration begins at *a* and finishes at *d;* that it offers *starts* or *jerkings* (*soubresauts*) in the proper sense of the word, corresponding to the line *a b*, a halting time analogous to that of inspiration; and, finally, a very long pause precedes the inspiratory movement of the succeeding respiration.

It may be objected that if this occurs in the thoracic walls during the contraction of the abdominal muscles, as the authors admit, the line *a b* should not present the irregularity it offers, and ought to continue the line of inspiration, which they term *active*, without interruption. But this objection is met by the remark that the digestive viscera are compressible, and that they can only act efficiently on the hypochondriæ when by their diminution in volume they have acquired sufficient elasticity to overcome that resulting from the displacement of the ribs. Otherwise, it might be considered, *à priori*, that the action of the abdominal viscera would be as prompt in coming into play as the inspiration is less deep—or, in other words, that the elasticity of the thoracic walls would be exercised in a more moderate manner.

The other tracings relating to "Broken Wind" in the third stage, have between them and the preceding the greatest analogy. Figs. 13, 14, and 15 are given as illustrations.



FIGS. 13, 14.—"BROKEN WIND" AT THE THIRD PERIOD.

It will be observed that the essential characters already insisted on are here reproduced, and, in addition, the tracing of Fig. 14 offers this peculiarity —that, after the production of the start, expiration is accomplished in one movement (*en un seul temps*); while inspiration, after commencing brusquely, is almost immediately interrupted by a start, then continues at a moderate rate to terminate very quickly.

In Fig. 10 only the thoracic tracing is given, whereas the thoracic and abdominal are shown in Figs. 13 and 14; in 15 the latter does not offer any special feature. This diagram is only remarkable as showing the brusque, but little extensive movement, which terminates expiration.

FIG. 15.—"BROKEN WIND" AT THE THIRD PERIOD.

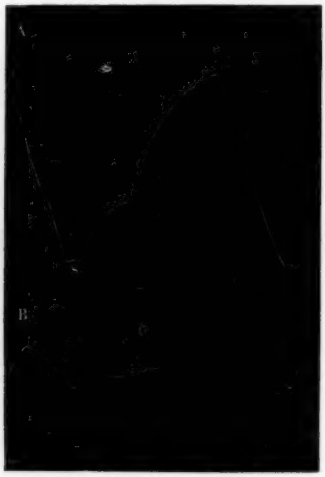


FIG. 16.—"BROKEN WIND" AT THE SECOND PERIOD.

THIRD TYPE.—"BROKEN WIND" AT THE SECOND PERIOD.—The third type corresponds to "Broken Wind" at the second period, and is represented by Fig. 16. The tracings in it (A thoracic, B abdominal tracing) were taken simultaneously on an animal destined for surgical purposes, and of which an autopsy was afterwards made, when general pulmonary emphysema at the second period was discovered. In the thoracic tracing there are two slight checks—one in inspiration, the other in expiration—as well as a start or jerk, *s*, at the commencement of expiration. To be assured that this start belongs to the second part of the respiratory movement, the former reason was invoked—the commencement of contraction of the abdominal muscles. But in this there could be no doubt that the break *s*, in the thoracic tracing, was not synchronous with the expiratory movement in the abdominal walls. The start is much less marked than that in the tracings belonging to the second type (third period); though it is noted that the abdominal muscles act with much less energy than in animals affected with "Broken Wind" at the third period, as is seen in the tracing B, allowing always for perhaps a slightly diminished sensibility in the pneumograph employed.

Fig. 17 contains a tracing which has the greatest resemblance to A in Fig. 16. There is a well marked halt in expiration, a less manifest interruption in inspiration, and as an essential fact, a jerk at the commencement of expiration.

"Broken Wind," a symptomatic expression of very varied lesions, has, then, three irreducible forms characteristic of the extent of the organic alteration.

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The first is betrayed by a period of rest, which most frequently occurs during expiration, but sometimes also interrupts the two respiratory movements ; it may even happen that it only takes place during inspiration. At the second period, the start is associated with the period of rest, but faintly. It appears most marked at the third period, though it does not exclude the rest interval.

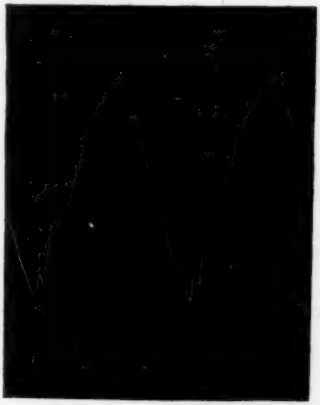

FIG. 17.—"BROKEN WIND" AT THE SECOND PERIOD.

The interpretation of these signs is readily furnished by a study of the tracings. The *rest interval* is a *suspension* of one or other, or even both, of the respiratory movements ; while the *start* or *jerk* is a *rebound* of the abdominal or thoracic parietes at the commencement of expiration, but it has not absolutely the same physiological signification in each of these regions. The jerk of the thoracic wall is a brusque elevation of the hypochondrium—a kind of very short, incomplete, and *passive* inspiration, intermediate to the two expiration movements, the first of which is more or less marked. The jerk of the abdominal walls or flank is much more easily observed than the preceding, and that which veterinary surgeons, as experts, look for ; it is due to the throwing back, towards the pelvis, by the thoracic expiration, of the digestive viscera previously displaced forward by the powerful contraction of the abdominal muscles.

In concluding their researches, MM. Laugeron and Laulanie add that they believe they have arrived at an exact determination of the symptomatic phenomena of "Broken Wind," and that they have dissipated a confusion, in which two facts essentially distinct were jumbled up together : the *period of rest* (*temps d'arrêt*) and the *start* or *jerk* (*soubresaut*). They acknowledge that an examination of their later tracings leads them to believe that they have not made sufficient allowance for the influence of the nervous system, in interpreting the rhythm of the respiratory movements in "Broken Wind." And even when it has been due to pulmonary emphysema, the rest-period does not appear to have received an explanation which may be deemed altogether satisfactory. But with a view to determine the exact part played by the nervous system in the production of the symptoms of "Broken Wind," the young physiologists have undertaken a series of experiments, the results of which they promise to furnish us with at a future period.

GOVERNMENT AID IN SUPPORT OF VETERINARY SCIENCE.

UNDER the above heading, the *Animal World* (the journal of the Royal Society for the Prevention of Cruelty to Animals) for last August makes the following welcome remarks:—

"From a humanitarian point of view nothing can be of more importance than the subject of a memorial prepared by Mr. Fleming, and recently passed the Royal College of Veterinary Surgeons, for presentation to the Government. It sets forth that, in order to increase the property of the nation in animals, as well by securing the best breeds as by multiplication of numbers, and the prevention of disease, veterinary science is indispensable. Contagious and infectious disorders, unless checked, may destroy all our animals, and epidemics produce an equal calamity if curative forces be not employed. Thus the food of the people, and the labour and pleasure we derive from horses, is jeopardised in a State which neglects to encourage veterinary knowledge and skill. Besides, as many of the diseases of animals are transmissible to human beings, and especially those which are eaten for food, it behoves the people to establish means of effectual protection, their own ignorance and inexperience being an insufficient safeguard. Every government in Europe except our own has taken veterinary science under its especial care, and has founded schools, several on a large scale, wherein cultivated, eminent men teach students at the public expense, it being accepted in those countries that thus the public advantage is greatly promoted.

"It also shows that up to the present time the profession has had serious and overwhelming difficulties to overcome, principally arising out of its pecuniary wants, and that it has yet received no help from the Government. At the same time the Royal College of Surgeons has received about £60,000 from the public purse, although this corporation is necessarily better able to provide for its wants than veterinary surgeons. It also states that the Royal College of Physicians hold premises under the Government at a nominal rent, while local habitations have been found for nearly every scientific society in London out of the public exchequer. It sufficiently shows that the veterinary science of England cannot keep pace with Continental progress, nor meet the demands of practice at home, unless help be given; and it prays the Government to grant it a building which would meet the requirements of the College (to be maintained by the College), or a sum of money sufficient to provide such premises.

"Can any one doubt the moderation, the reasonableness, the importance of this modest memorial? Can any one doubt that it ought to be and will be granted immediately it is properly placed before the Government? The great agitation recently carried on in the newspapers, and the bill of the Government just made into a statute, are presumably designed to protect animals in this country from disease. Have not veterinarians during the last ten years insisted on the wisdom of this legislation, as being second in importance only to the entire prohibition of foreign animals, except as dead meat—which is the best remedy in our opinion, and one which the Government measure will no doubt promote? And if the most eminent men in veterinary science have contributed to sound policy by their warnings and counsels, surely the country will recognise the claims of the profession which now seeks public aid to continue its good work for animals and for mankind. We cannot benefit animals without *pro tanto* benefiting their owners. Considered, therefore, on the lowest grounds of property, the proposal should have hearty support.

"But there are much higher grounds—civilisation and humanity—from which to view the subject. The status of the veterinary surgeon needs to be

raised higher for his own good, and for the better treatment of animals. As the life and sensitiveness of an animal are regarded by public opinion, in the same proportion will the status of veterinarians be advanced or lowered; just in proportion with the elevation of that profession will superior men enter it, for the standard of education will also proportionally be raised. Need we add that in the same proportion will the welfare of animals be advanced?—for the experience of all persons has led them to beware of ignorant 'horse-leeches' and uninstructed 'farriers,' and to find in educated veterinarians the best guardians of our dumb companions. Surely the R. S. P. C. A. has from its foundation to the present time much to be grateful for in the support given to it by leading veterinarians, and much to hope for in the future from the development of the Royal College of Veterinary Surgeons, as projected in this memorial. We ask all our wealthy members at once to throw in the scale the weight of their influence, by inducing members of Parliament to support the memorial, and by pressing the members of the Cabinet to accede to its prayer. The opposition given by our veterinary surgeons as a body, and particularly by all its chief members, to the cruel practices of vivisection at Alfort, and generally to the abuses of that mode of advancing medical knowledge, deserves to be remembered at this juncture, and to call out every generous impulse of lovers of animals on their behalf.

"For the above reasons the Royal Society for the Prevention of Cruelty to Animals has always given its best wishes and moral support to veterinarians, and will use its influence in the furtherance of the present excellent movement."

SUPPOSED POISONING BY "MOULDY" FODDER.

In a recent number of the *Wochenschrift für Thierheilkunde und Viehzucht*, Veterinary Surgeon Anton Herele reports an interesting case of fungus-poisoning of oxen which came under his notice in his capacity of district veterinary surgeon during the summer of the present year. On May 21st he was called upon to visit seven beasts suffering more or less severely from illness of an apparently mysterious nature. All of them had lost flesh considerably, in proportion to the severity of the attack in each case; they stood with their backs arched and necks twisted on one side; saliva dribbled freely from their mouths; and the movements of rumination were constant, though no food entered the œsophagus. From time to time a hoarse cough was heard, which was obviously due to the presence of food in the pharynx irritating the epiglottis, which could not be got rid of owing to partial paralysis of the organs of deglutition. When disturbed the beasts rose up leisurely, and voided a small quantity of hard, slimy fæces, straining much in the effort. The urine occasionally escaped involuntarily. In ten days' time the cows most severely attacked were reduced to a state of extreme debility accompanied by total paralysis of the hind-quarters, and were then slaughtered, fifteen days after the commencement of the disease. During the whole illness there was only very slight acceleration of the pulse, and no disturbance of sensibility, but the temperature of the skin fell very low, a *post-mortem* examination throwing no light on the cause of the disease. Herele proceeded to examine the food, in which he found a good deal of old spelt and barley straw mixed up with young green clover. The spelt was infested with *Tilletia caries*, and the barley straw with ordinary *Puccinia graminis*; and to their presence, in the absence of any other reasonable explanation, he is inclined to attribute the outbreak of disease. The diet of the surviving

beasts was at once changed for green clover only; and this alteration, with the addition of a little hydrochloric acid to their water, and free ventilation of the stalls, was followed by the complete recovery of all in about three weeks' time, though at first the improvement was extremely slow and slight.

THE PRESENT STATE OF OUR KNOWLEDGE WITH REGARD TO THE INTIMATE NATURE OF INFECTION AND CONTAGION, AND ITS RELATION TO THE PREVENTION AND CURE OF ZYMOTIC DISEASES.

THE following is the second of two lectures delivered by Dr. T. G. Grimshaw, of Dublin, before the King and Queen's College of Physicians in March last, and published in the *Dublin Journal of Medical Science* for August. It affords an excellent and lucid account of the present state of medical knowledge with regard to infection and contagion and the prevention of zymotic diseases, and as such is deserving of the careful attention of veterinary surgeons.

CONTAGIUM OF SPECIFIC FEVERS.

In considering the nature of the contagium of specific diseases, we have to face the question of spontaneous generation; and just as this question bears upon the production of septic poison, so it bears also upon specific contagium. Thus, according to one view, contagium may originate *de novo;* according to the other, it can only be derived from previously established disease or the germs thereof. Those who disbelieve in spontaneous generation believe in what has been conveniently termed "contagium vivum," which, strictly interpreted, implies "seed of disease," and therefore assume the pre-existence of a parent. "Contagium vivum" and septic poison are not, in any sense, convertible terms, although many have used them as synonyms, and hence many mistakes. I have shown that "septic poison" is not a living, but the product of a living, organism, is incapable of self-multiplication, and resembles the poisonous principles of vegetables. On the other hand, contagium, whether producible *de novo*, as the advocates of spontaneous generation have it, or the descendant of a remote ancestry, as their opponents hold, presents all the properties of living organisms. I have already assigned my reason for considering that "spontaneous generation" has not yet been proved; and, therefore, it is unnecessary to discuss the question again in connexion with specific contagium. Although the origin *de novo* of specific diseases has not been proved, yet it is affirmed by many that contagium can be produced and cultivated outside living bodies, and many attempts of a very substantial and ingenious nature have been made to prove the essential connexion of certain specific forms of zymotic diseases with organisms which may be cultivated outside the bodies of living animals. To the consideration of this question I shall now address myself.

There is but one contagious febrile affection to which human beings are iable which affords a fair and justifiable field for experiment; and that disease is Cow-pock, which must be considered as a variety of Small-pox, and its contagium as Small-pox contagium, modified by cultivation, just as the species of *Brassica*, especially *Brassica oleracea*, have been modified into our various table vegetables—cabbages, kale, greens, cauliflowers, broccoli, etc. That a "contagium vivum" should be so modified is not surprising, wnen we consider the ease with which the gardener modifies vegetables. The surprising fact

is that, up to the present, the contagium of Small-pox is the only one which we have been able to modify in a *certain* and favourable manner. It thus happens that not only nearly all our exact knowledge of the intimate nature of specific contagium has been inferred from the study of Small-pox and Vaccinia, but, also, that many of the misconceptions which still prevail, relative to the physical conditions of contagion, have been inferred from superficial observations upon these diseases. Our familiarity with Vaccinia and Variola being so great, it was natural that the first attempt to investigate the intimate nature of contagion should be directed to these diseases. Accordingly, we find that, so long ago as the year 1863, Dr. Beale* discovered, in perfectly clear vaccine lymph, certain little particles; and these little bodies Dr. Beale terms (in accordance with the views which he holds concerning the simplest form of life) "germinal matter," or "bioplasm." About the same time Dr. Chauveau,† of Lyons, commenced an independent series of observations upon Small-pox and vaccination. His first observations were directed to the determination of the question as to whether the contagium of these affections was solid, fluid, or gaseous, or capable of being brought into these states. The result of Chauveau's experiments was to prove that the contagium of Vaccinia was insoluble, and that water brought in contact with vaccine lymph did not become impregnated with the virus of the lymph. He also experimented with fluid from Small-pox and from Sheep-pox, the result in every case being similar. Dr. Sanderson‡ has repeated Chauveau's experiment with additional precautions, and obtained exactly similar results. Thus it has been proved that contagium is *particulate*. It has also been proved that the particles are extremely minute—for vaccine matter may be diluted ten times without losing its power, but increased dilutions diminish the certainty of its action. The virus of Sheep-pox may be diluted by 10,000 times its bulk of water and still produce its effects. Inoculations with diluted lymph, if they take at all, produce quite as typical results as operations performed with undiluted lymph, showing that the introduction of the contagium in the smallest quantity will produce the characteristic effects. These observations have been further confirmed by experiments upon other diseases of animals. Contagium, therefore, appears to be neither soluble nor volatile, its apparent solubility depending upon the minuteness of its particles, which can be easily diffused through a fluid, and will remain suspended for a considerable time, just like the impurities which I mentioned as existing in fluids, and only capable of demonstration by an electric beam. In the same manner contagium may be suspended in the air, just as the motes which are seen floating in the sunbeam. Tyndall's experiments showed how bodies which are neither volatile nor soluble may be so suspended in air or fluid as to give the idea of volatility or solubility, and so solved the difficulty which existed in accounting for the communication of infection through the atmosphere.

Accepting it as proved that contagium is "particulate," our next inquiry is—What is the nature of the particles? Are they living or dead? And, if living, are they animal or vegetable?

The evidence that the particles are living rests upon this—that they conduct themselves as only living organisms do—namely, when placed on a suitable soil, they grow and have the power of self-multiplication to an indefinite degree. In this they differ from the septic poison, and from all other poisons, whether of organic or inorganic origin. It is easier to prove the vitality of

* *Quarterly Journal of Microscopical Science*, April, 1864.

† "Vaccine et Variole: nouvelle étude expérimentale sur la question de l'identité de ces deux affections."—Paris, 1865. "Détermination expérimentale des éléments qui constituent le principe actif de la sérosité vaccinale virulente."—*Comptes Rendus*, LXVIII. (1868).

‡ Twelfth Report of the Medical Officer of the Privy Council (1869).

the particles of contagium than to decide to which of the two great kingdoms of living organisms they belong. There is no crucial test yet known which can be so applied as to determine whether one of the lowest organisms is animal or vegetable in its nature. As the minute beings, known in the early days of microscopy as "infusoria," were found to be endowed with the power of motion, it was assumed that all such moving organisms were minute animals; but it is now known that many vegetables are endowed with the power of motion, and even devour animals; therefore, this distinction between plants and animals is no longer tenable. The chief test between animal and vegetable growths depends upon the nature of their reproduction and development; and, applying this test, it is found that the organisms associated with the propagation of contagium are of a vegetable nature and probably belong to either the class *fungi* or *algæ*. Many contagious cutaneous diseases are now known to depend upon the growth of fungi; such diseases, however, are but slightly associated with febrile conditions, or acute destructive tissue changes, such as accompany the growth of the organisms connected with contagious Fevers.

The investigation of this subject is one surrounded with the greatest difficulties, and requiring most careful observation. When I state that Dr. Klein,* who is one of the most accurate observers of the day, made the mistake of thinking he had discovered a fungus growth in connection with Sheep-pox, which he afterwards ascertained was nothing more than albumen, altered by the process of preparing the specimen for microscopic inspection,† I think I say enough to show the care required in founding hypotheses even on observed facts. I shall now, as briefly as I can, refer to the present state of knowledge with regard to the low organisms which accompany the development of contagion. The first attempts to connect the development of fungi with any specific form of zymotic diseases seems to have been made by Dr. Cowdell, of Dorchester, who suggested the idea in 1848. In 1849, Drs. Brittan and Swayne, of Bristol,‡ believed that they had discovered vegetable organisms which bore some relation to Cholera. I think the first substantial attempt to connect the origin of any specific febrile zymotic with a fungoid growth, was made by Dr. Salisbury, of Newark, Ohio, in 1862,§ who suggested that the origin of Camp Measles was connected, by the contact of those attacked with damp straw upon which certain fungi (*puccinia graminis*) had been developed. Following up his inquiry, Dr. Salisbury discovered that he could by inoculation with the spores of those fungi produce a disease very similar to, if not identical with, Measles—and which conferred immunity from other attacks of Measles. Although Dr. Salisbury has not produced any evidence of the growth of these fungi in the human body, he has given very substantial proof of their power to produce Measles; but he has not proved that the contagium of Measles is identical with the fungus or its spores produced on damp straw.‖ Dr. Salisbury's experiments do not seem to have attracted that notice which they deserve; they have been passed by, by some, as observations not yet confirmed: but I am not aware that any successful attempt has been made to disprove Dr. Salisbury's conclusions. Dr. Tilbury Fox evidently accepts the facts published by Dr. Salisbury.¶

* Report of the Medical Officer of the Privy Council and Local Government Board. New Series. No. III. (1874).

† Ibid. No. VIII. (1876).

‡ *London Medical Gazette*, and *Lancet*.

§ *The American Journal of the Medical Sciences*, July, 1862, and October, 1862.

‖ "The Action of Vegetable Fungi in the Production of Measles and Allied Diseases." See also Dr. Tilbury Fox in *Journal of Social Science*, Vol. i., No. III.

¶ *Loc. cit.*

The most systematic attempt of the kind was made by Dr. Hallier,* of Jena, during the Cholera epidemic of 1866-67. He believed he had proved that the discharges and vomit from Cholera patients contained certain fungi and their germs; these he cultivated outside the body, and went so far as to identify them with a fungus met with on rice grown in India, where Cholera seems to take its origin. Hallier's explanation of the presence of these fungi seems to be that these bodies (*Urocystis*) were incapable of growth in northern climates, on account of the low temperature, but that the bowels of infected persons provide the necessary climate. It has, however, since been shown by Drs. Cunningham and Lewis† that the organisms which Hallier connected with Cholera do not exist in connexion with rice, and that Hallier's fungi have no necessary connexion with Cholera. Other fungoid bodies have been met with in Cholera discharges; but, as yet, none can be proved to be the promoting cause of that disease. With regard to Small-pox, Dr. Keber, of Dantzig, 1868, discovered organisms in the lymph of Variola and Vaccinia which he considers to be either the generators or the carriers of the contagium of the disease. These bodies are now known as *Micrococci*. Dr. Sanderson observed bodies in vaccine lymph, in 1870, which he called *Microzymes*. Cohn found that these organisms were common to both vaccine and variolous lymph; and besides these other circular bodies which he considered to be cells, and frequently united in pairs like the figure 8, which, on keeping for a time, developed into chains and necklaces, then again aggregated into colonies or clumps, and became united by a mucoid substance into *Zooglœa*.

Weigert, of Breslau, found in the lymphatic vessels of persons who had died from Small-pox masses of *Micrococci;* these were only found on the sixth or seventh day. After that the bodies became indistinct; and as few die before the sixth or seventh day of Small-pox, it was impossible to follow the life-history of these bodies in the human subject, or to prove their necessary connexion with Small-pox. However, as Sheep-pox, or *Variola ovina*, is so exactly similar to human Small-pox, Dr. Klein undertook to follow the question up by observations upon that disease. Unfortunately Dr. Klein fell into a reasonable error with regard to the nature of the bodies, of which he published an account in 1874, and in 1876 he acknowledged his mistake; nevertheless, the presence of the *Micrococci* described by Weigert, and again by Dr. Klein, has not been gainsaid, and must be taken as an established fact. I shall be mistaken if Dr. Klein does not pursue his inquiries upon this question to a successful issue. In the case of the Splenic Fever of cattle, now known to be capable of attacking man, certain bodies have been discovered which are proved to have a necessary connexion with the disease.‡ They are found in the blood, and the blood of an infected animal will communicate the disease to a healthy animal. These bodies are in the form of rods, capable of growth, and produce spores in their interior, which are set free by the breaking up of the wall of the rod-like bodies. The presence of these bodies is necessary to the communication of the Fever by infection. While the rod-like bodies easily lose their vitality, the spores produced by them are capable of resisting boiling temperatures, and will not alter even in the presence of oxygen under high pressure.§ Thus, in Splenic Fever, the question of the necessary existence of specific organisms is settled

* "Das Cholera Contagium": Leipzig, 1867. See also Dr. Buchanan in Ninth Report of the Medical Officer of the Privy Council (1867).

† Sixth Annual Report of the Sanitary Commissioner with the Government of India: Calcutta, 1870. Also Report of the Microscopical and Physiological Researches into the Nature of the Agent or Agents producing Cholera: Calcutta, 1872. Report on Cholera: Calcutta, 1874.

‡ See Dr. Sanderson's Lectures, *loc. cit.*

§ Pasteur and Joubert, "Bulletin de l'Académie de Médecine" (1877).

beyond a doubt. In cases of Diphtheria and Erysipelas, *Micrococci* are found in all infected parts; but it has not been proved that these bodies are essentially connected with these diseases. In Relapsing Fever bodies called *Spirillæ* * have been discovered, which seem to have an essential connexion with this disease. These bodies are little spiral filaments, which are found only during a paroxysm of the Fever, and are always absent during an apyrexial period.

In all those diseases in which *Micrococci* have been demonstrated as occurring, certain conditions, known as Ulcerative Endocarditis, have been discovered in the lining membrane of the heart. This condition has been met with in Small-pox, Scarlatina, Measles, Diphtheria, Erysipelas, and Pyæmia. A close examination of these endocardial affections shows them to consist of colonies of *Micrococci*. These in many cases become broken up, and are disseminated through the body, and become the foci of little nodules. They have themselves been derived from the site of the original infection, and in their turn become infecting agents. Now, although all these various organisms have been found in connexion with specific infective diseases, yet it still remains an open question whether any of them are particles of contagium. In the cases of Splenic Fever and Relapsing Fever, the organisms appear to have the necessary connexion with the diseased conditions, but such connexion is wanting in the other affections referred to. Dr. Beale,† some five years ago, rejected all the vegetable theories of contagious zymotics. The evidence then in their favour was not so strong as now, yet nevertheless Dr. Beale's views have not been controverted, and his idea of a living matter, which he terms "bioplasm," capable of modification into the various forms of specific contagion, commends itself at once as a view likely to reconcile many differences and be acceptable to practical as well as inductive physiologists, pathologists, and physicians.

Conclusions as to the Present State of our Knowledge.

A.—With regard to fermentation: 1. That certain kinds of fermentation, in which *Bacteria* take part, are capable of developing a poison, called the septic poison, which produces specific effects when introduced into the circulatory system; 2. That the septic poison may be produced independent of the living body, or may be elaborated in wounds by the action of *Bacteria*.

B.—With regard to spontaneous generation: 1. That the spontaneous generation of living organisms has not yet been proved; 2. That spontaneous generation is not impossible; 3. That in the absence of proof or disproof of spontaneous generation, we must act as if such a process were possible.

C.—With regard to septic infection: 1. That the septic poison produced by *Bacteria* and their progeny, when introduced into the system, produces disintegration of the blood corpuscles, consequent coagulation of the blood, and thus stasis and capillary congestion; 2. That the effects of the septic poison are in direct proportion to the dose.

D.—With regard to the contagion of specific diseases: 1. That the contagia of specific diseases are particulate and living; 2. That these contagia closely resemble vegetable organisms in their growth and development; 3. That up to the present the specific contagium of any disease has not been demonstrated; 4. That in several contagious diseases certain organisms

* Obermeier, *Berlin. Klin. Wochenschrift*, 1873. See also Sanderson, Report of Medical Officer of Privy Council and Local Government Board. New Series, No. III.

† "Disease Germs: their Nature and Origin." 1872.

have been found which are constantly related to those diseases, and must therefore be considered to be either the contagia of such diseases or the only carriers of such contagia.

Practical Application.

I shall now proceed to make some practical application of the knowledge which I have shown we possess upon this subject.

With reference to *septic infection*, assuming that it is true that the septic poison is the product of the growth of *Bacteria*, it is quite evident that any agent which will kill the *Bacteria* or their germs before they have developed the poison will, as a matter of necessity, prevent the occurrence of septic infection. Now, I have shown that septic poison may be produced outside and at a distance from the body which it ultimately affects; and that, further, all the conditions favourable to the growth of *Bacteria*, and therefore to the production of septic poison, exists in wounds and sores; and that the intestinal tract itself contains *Bacteria* capable of producing septic poison in the neighbouring parts, if the proper soil is provided. We at present know of no antidote to the septic poison; and our dealings with it, when once it enters the system, consist in maintaining the life of our patient until the effects of the poison have worn off, or combating the various secondary conditions which septic infection gives rise to. We may, however, prevent the entrance of the poison, or even prevent its formation, under certain circumstances. The greatest difficulty with which the surgeon has to contend is the septic infection of wounds. The results of the most carefully devised and beautifully executed operations have been frustrated by septic infection. I think I have shown conclusively that ordinary air contains, among many floating objects, the germs of the lowest forms of life, and among these *Bacteria* and their germs. This being admitted, it follows that a wound exposed to the influence of ordinary air is at the same time exposed to the danger of becoming a field for the growth of *Bacteria;* and if these *Bacteria* settle upon the surfaces of the wound and there produce their venom, the patient will as certainly suffer as if the septic poison itself, produced from a putrid infusion, had been introduced into the wound. The avoidance of the introduction of matter known to be putrid, and therefore possibly containing septic poison, is a mere matter of scrupulous cleanliness, and the carrying out of such cleanliness is well known to be a source of great success in surgery. All putrid matters should, therefore, be removed far from the healthy, and, still more, from the sick; and in public institutions and hospitals should be destroyed by fire or powerful corrosive chemical agents. We cannot, however, apply corrosives or fire to our patients and their wounds, and fortunately, here we have the means of preventing the formation of septic poison by the use of antiseptics. These antiseptics act, not by destroying the poison itself, but by destroying the agents which produce the poison. The poison has been shown (Dr. Anders) to be capable of withstanding the most powerful antiseptics.

When speaking of fermentation, I cited the case of putrefaction as an instance of fermentation. It is right here to state that some authorities have endeavoured to distinguish between fermentation and putrefaction, defining the former as changes connected with the growth of "torulæ" (yeast plants), and the latter as changes in connexion with *Bacteria*. Now, this I consider quite an artificial and misleading distinction, and one which, if followed out, would lead to the giving of special names to a variety of processes almost identical with torulous fermentation. This attempt to distinguish between fermentation and putrefaction seems to have followed on the discovery that certain agents which control torulous fermentation do not control bacterial fermentation. It is with bacterial or septic fermentation that we have to deal

when considering the question of antiseptics, which are agents fatal to the growth of *Bacteria*. The test of the power of an antiseptic is the amount of control it has in preventing the occurrence of septic fermentation in a cultivating fluid. Experiments carried out by Dr. Baxter,* published in 1875, have shown the relative antiseptic powers of different agents. Dr. Baxter's method of taking antiseptic power is as follows :—"A known portion of the agent under investigation was added to a liquid teeming with septic microzymes. A test solution, previously sterilised by heat, was then inoculated with a minute drop of the disinfected liquid. If it continued barren, the successful destruction of the septic germs was proved. In the event of their incomplete destruction, the test liquid was found crowded with their progeny." The result of Dr. Baxter's experiments was to prove that—

	Strength per cent. or more.	
Potassic permanganate	0·007	destroyed the reproductive power of microzymes.
Chlorine . . .	0·0008	
Sulphur dioxide .	0·123	
Carbolic acid . .	1·	

The antiseptic action of the permanganate and chlorine is seriously impaired by the presence of albuminous compounds, whereas the action of sulphur dioxide and carbolic acid are scarcely, if at all, so affected. Hence, in dealing with animal fluids, the two latter are the valuable agents, and the two former nearly useless.

Bucholtz found that antiseptic agents prevented the formation of *Bacteria* when used in the preparations set forth in the accompanying table, which has been kindly furnished by Dr. Burdon Sanderson for the purpose of illustrating these lectures :—

Table showing the power certain agents have of preventing the development of or destroying the growth of Bacteria.

Arresting the Development of Bacteria. Name of Disinfectant.	Strength of solution found necessary.	Rendering existing Bacteria incapable of further Growth or Propagation. Name of Disinfectant.	Strength of solution found necessary.
	One in		One in
Corrosive Sublimate .	20,000	Chlorine . .	25,000
Thymol . . .	2,000	Iodine . . .	5,000
Sodic Benzoate . .	2,000	Bromine. . .	3·333
Creosote . . .	1,000	Sulphurous Acid .	666
Carvol . . .	1,000	Salicylic Acid. .	312
Benzoic Acid . .	1,000	Benzoic Acid . .	250
Salicylic Acid . .	666	Thymol . . .	200
Eucalyptol. . .	666	Carvol . . .	200
Sodic Salicylate. .	250	Creosote . .	100
Carbolic Acid . .	200	Carbolic Acid .	25
Quinine . . .	200	Alcohol . . .	4
Alcohol . . .	50		

An important paper on this subject was published in *The Dublin Medical Journal* for April 1876, p. 321, by Edwin Lapper, licentiate of this College. Acting upon information of this kind, Professor Lister, by the careful and

* Report of the Medical Officer of the Privy Council and Local Government Board. New Series, No. VI., 1875.

assiduous applications of these scientific facts to practical surgery, has devised a method of treating wounds now known as "antiseptic surgery," or, as it may be fairly called, "Lister's method," for to him belongs the credit (in spite of arguments about priority of suggestion) of having really applied the science of antiseptics to practical surgery. I cannot enter upon the details of Lister's treatment of wounds, but it consists in taking measures to destroy as completely as possible all *bacteria* and their germs, before either they or their septic venom have established themselves in the wounds produced by operations and accidents. This is accomplished by surrounding the patient with an antiseptic atmosphere, bathing the wounds with antiseptic fluids, and forming a screen of antiseptic material to prevent the access of *microzymes* to the wounded part. I am aware that antiseptic surgery is not always successful, and that it is stated that *microzymes* have been found behind Lister's antiseptic screens. I am also aware that very successful results have been obtained by other means than those adopted by Lister. Antiseptic surgery has been called a system merely of cleanliness : be it so, the dirt to be dealt with is dangerous, and moreover *living*. Lister has proved that he can with comparative ease destroy this dirt, and can show why it is dangerous, and that it is destroyed by depriving it of life. This is undoubtedly the most efficacious way of disposing of these pests of surgery, and thus destroying septic infection and its possible results—pyæmia and hospital gangrene. The experience of Continental surgeons has fully confirmed Lister's views and practice. So far the practical application of our knowledge with regard to septic fermentation and the production of the septic poison is found in antiseptic surgery. May not the physician also apply this knowledge to the prevention of septic infection from the ulcers of Enteric Fever, the sloughing sores of the throat in Scarlatina or Diphtheria, or the condition of skin in the advanced stage of Small-pox? I believe he can, though perhaps to a more limited degree than the surgeon ; yet the practice of the surgeon, in this as in many other cases, may serve as a useful guide to the physician.

We have now to consider the application of our knowledge to the prevention and cure of specific contagious zymotics. I cannot consider each disease of this class, but I shall endeavour to point out generally, and in some cases particularly, how these diseases are to be dealt with, and in doing this we must view the question from the following points :—

1. The modification of the nature of contagium.
2. The destruction of contagium—*a.* without the body ; *b.* within the body.
3. To counteract the effects of contagium.

1. But little way has yet been made towards the modification of the nature of contagium. In fact, the only contagium which we have yet been able to modify is that of Small-pox, and our power to modify that contagium has reached an amount of exactness and certainty which affords the strongest proof of the value of experimental pathology, and the greatest encouragement to follow up those investigations (which are now becoming so common) into the nature of infectious diseases among animals, and their relation to human complaints. The results of vaccination are so well known, and the benefits it has conferred on mankind so extensive, that it is quite unnecessary for me to refer to them further.

2. The means at our disposal for the destruction of contagium are numerous and various. They differ so much in their mode of application, and the circumstances under which each may be used are so unlike, that we are frequently unable effectually to apply any of them.

The agents termed disinfectants, which are used for the destruction of contagium, must not be confounded with antiseptics, which prevent septic fermentation by the destruction of *microzymes*, etc. There is a strong *probability* that an antiseptic will prove a disinfectant, but that is all ; it is

not necessarily so, nor *vice versâ*. I have shown already that the power of some antiseptics is diminished by the presence of albuminous compounds; and as contagious particles are frequently, some say always, associated with albuminous substances, the power of these antiseptics must be thus diminished. Contagium within the system must of necessity be associated with albuminoid substances, and therefore possess additional protection against the action of antiseptics.

I shall now consider the action of disinfectants on contagium, and here again we must be chiefly guided by the results of Dr. Baxter's experiments. Dr. Baxter * adopts as a definition of disinfectant "any agent capable of so modifying the contagium of a communicable disease, during its transit from a sick to a healthy individual, as to deprive it of its specific power of infecting the latter." It will be observed that this definition excludes the destruction of contagium within the body. The particular forms of contagium selected for experiment by Dr. Baxter were vaccine, the virus of infective inflammation, and the virus of Glanders. The disinfectants tested were potassic permanganate, sulphur dioxide, chlorine, and carbolic acid—the same used in his experiments on septic fermentation; also exposure to heat. The results of the experiments prove that—

a. Carbolic acid, sulphur dioxide, potassic permanganate, and chlorine, are all endowed with disinfectant properties.

b. The effectual disinfectant operation of chlorine and potassic permanganate depends more on the nature of the medium in which the contagious particles are contained than upon the nature of the particles themselves: thus, when the contagious particles are protected by the presence of organic compounds, or when agents capable of combining with chlorine or decomposing the permanganate are present, the powers of these disinfectants are so impaired that their action cannot be relied upon.

c. A virulent liquid cannot be regarded as certainly and completely disinfected by sulphur dioxide, unless it has been rendered permanently and strongly acid.

d. No liquid can be considered disinfected by carbolic acid, unless it contain 2 per cent. by weight of the acid.

e. When disinfectants are mixed with a liquid, it is important to be sure that they are thoroughly incorporated with it; that no solid matters capable of shielding contagium from immediate contact with its destroyer be overlooked.

f. Aerial disinfection, as commonly practised in the sick room, is either useless or positively objectionable, owing to the false sense of security it is calculated to produce. To make the air of a room smell strongly of carbolic acid, by scattering carbolic powder about the floor, or of chlorine, by placing a tray of chloride of lime in a corner, is, so far as the destruction of specific contagia is concerned, an utterly futile proceeding. When aerial disinfection is resorted to, the probability that the virulent particles are shielded by an envelope of dried albuminous matter should always be held before the mind. Chlorine and sulphur dioxide are, both of them, suitable agents for the purpose; the latter seems to be the more effectual of the two. The use of carbolic vapour should be abandoned, owing to the relative feebleness and uncertainty of its action.

g. Dry heat, when it can be applied, is probably the most efficient of all disinfectants. But, in the first place, we must be sure that the desired temperature is actually reached by every particle of matter included in the heated space; secondly, length of exposure and degree of heat should be regarded as mutually compensatory factors within certain limits. The above statements are not so discouraging as they may appear at the first

* *Loc. cit.*

glance to our reliance upon artificial disinfection. If we believe that all contagia are generated, like those of Small-pox and Scarlet Fever, in the infected organism, and there only, the outlook is a hopeful one. We might even anticipate an approach to the *perfect* fulfilment of the work of disinfection, by subjecting all matters immediately after their removal from the affected person, and before any dilution or admixture, to the full influence of one or other among the destructive agencies at our command. On the other hand, if the contagium of any disease is capable of being generated *de novo* outside the body (pythogenic origin of Enteric Fever, Typhus created by overcrowding), such contagium can hardly be eradicated by any method of artificial disinfection. For cases of the latter kind the opening words of the memorandum previously referred to furnish the only solution—namely, it is to cleanliness, ventilation, and drainage, and the use of perfectly pure drinking water, that populations ought mainly to look for safety against nuisance and infection. Artificial disinfectants cannot properly supply the place of these essentials, for, except in a small and peculiar class of cases, they are of temporary and imperfect usefulness.

The above quotations show the important relationship between the question of spontaneous generation and the destruction of contagium by disinfection. It will be seen that the operation of chemical disinfectants is very uncertain, and the disinfecting power of heat is very certain, as shown by experiments performed by Dr. Baxter, who performed ten series of vaccinations, each with six points; three of the points were heated; three were used in the virgin state. The results are shown in the following tabular statement, which shows, in columns, how many of the vaccinations were successful :—

No. of Experiment.	Maximal and Minimal Temperature in degrees, Fahr.	Results.	
		Arm operated on with virgin points.	Arm operated on with points heated as denoted in column 2.
	° °		
1	134·6 to 138·2	2	2
2	140· ,, 145·4	3	3
3	145·4 ,, 150·8	2	3
4	152·6 ,, 158	2	1
5	156·2 ,, 165·2	3	3
6	167 ,, 176	3	3
7	185 ,, 194	Unknown	Unknown
8	— ,, —	3	0
9	194 ,, 203	2	0
10	— ,, —	3	0

It will be observed that until the temperature to which the vaccine had been exposed exceeded 194° Fahr., its infective power was not destroyed.

In dealing with this question, it must be remembered that ordinary fabrics will not bear a temperature above 260° Fahr. without injury. Nor is it yet proved that this temperature will in all cases destroy organic life; and it is well known that some portions of the clothing must be exposed to a temperature quite unnecessary to the destruction of life, in order that other portions may be sufficiently heated; therefore it is decidedly better in all cases to destroy infected clothing by fire. Now, having stated so much

with regard to the means we possess of destroying contagium *outside* the body, the next point to be considered is—what power have we of destroying contagium within the body? This is a question beset with many difficulties. It is quite clear that if we are to rely on disinfectants, these must be introduced into the system in sufficient quantity to cause the death of the living particles of contagium; and in doing this we must bear in mind that we do not cause the death of our patient by poisoning him with our remedies, or that we do not seriously damage or destroy some of the delicate organs which are necessary to his healthy existence. Dr. Baxter and others have shown that the presence of albumen, or bodies having a great chemical affinity for chlorine or oxygen, prevent or counteract the effect of disinfectants such as chlorine and potassic permanganate, and in a less degree sulphur dioxide. We cannot, therefore, hope that any benefit would accrue if we were able to introduce these bodies into the circulation in considerable quantities. In addition to this, we have to consider that, in the case of carbolic acid, which seems to be a powerful disinfectant, not much, if at all, impaired in its powers by the presence of organic compounds, the amount (2 per cent.) required to produce disinfection is too great to be introduced into the system. It is known that if carbolic acid is applied over a very extensive raw surface, it may produce poisonous effects. I have myself considered it necessary to be very careful in the use of carbolic acid as a local application in Small-pox on this account. We dare not, if we could, introduce 2 per cent. of carbolic acid into the circulation of a human being. Fortunately, we are possessed of other disinfectants whose powers are not easily impaired by the presence of organic matter: these are quinine, salicylic acid, and *possibly* also thymol, carvol, eucalyptol, and other recently investigated antiseptics and disinfectants. The power of these agents to destroy contagium has not yet been properly tested, but estimating their disinfectant by their known antiseptic powers (see Table, p. 264), we have good reason to conclude that they are disinfectants. Quinine and salicylic acid possess this advantage—that they can be introduced into the system in large quantities without injury to the patient. We have, therefore, two drugs which have the power of acting as disinfectants within the system. This power, indeed, seems to have been to a certain extent demonstrated in the case of these drugs. We know that the effect of the action of any contagium within the system is to produce fever, and that the essential symptom of fever is a high temperature. We know that quinine and salicylic acid, administered in large doses, reduce temperature—in fact, suspend for a time the febrile processes—and the continued use of these drugs reduces fever. We know further, from the experiments of Professor Binz, of Bonn,* that quinine possesses the power of diminishing, and, in the proportion of 1 in 4000, absolutely destroying, the motive power of white blood-corpuscles, or, as Dr. Beale would term them, the bioplasts or bioplasm of the blood. Without absolutely adopting Beale's views, I think he has offered strong evidence in proof of the view that all particles of contagium are bioplasm which has undergone certain modifications—in fact, he has to some extent shown it in the cases of Variola and Vaccinia. We may, therefore, fairly draw the inference that an agent which has a powerful action on the bioplasm of the blood or *bacteria*, and which produces results (namely, the reduction of fever) which would follow if it acted on the bioplasm of contagium within the system, has the power of acting as a disinfectant within the system. Such an agent *is* quinine, and to it and similar agents we must look for the power to control contagium within the body. The observations of Dr. Lapper, to which I have referred, point to the fact that certain drugs, when mixed with dead blood (in such proportions as could be introduced into living blood

* *Practitioner*, vol. ix. (1872).

without danger to the patient), have the power of retarding decomposition. These experiments are corroborative of the view that we do possess disinfecting agents capable of acting within the system. I think, therefore, we may fairly conclude that we can not only destroy contagium *without*, but also within the body. The consideration of the counteraction of the effects of contagium would lead us into the discussion of the treatment of all the abnormal conditions that arise as a result of febrile processes set going by the growth of contagium within the system; but in such limited time it is impossible to follow all the details of so large a question. However, I will refer to a few. In Dr. Sanderson's Report on the Processes of Fever, he says :—

"A satisfactory explanation of the nature of fever and of its relation to the febrile process is not at present possible, because we are not as yet possessed of the necessary physiological knowledge. We have elsewhere stated that two possibilities are open to us. One is that fever originates in disorder of the nervous centres; that, by means of the influence of the nervous system on the systemic functions, the liberation of heat at the surface of the body is controlled or restrained, so that by retention the temperature rises; and finally, that the increased temperature so produced acts on the living substance of the body so as to disorder its nutrition. The other alternative is that fever originates in the living tissues; that it is from first to last a disorder of protoplasm, and that all the systemic disturbances are secondary.

"In both hypotheses it is tacitly assumed that fever is the product of a material fever-producing cause, contained in the blood or tissue juice, the morbific action of which on the organism is antecedent to all functional disturbances whatever. At bottom we are all humoralists, and believe in infection. It is not until we have to say when and how the infection acts that questions arise. The facts and considerations we have had before us are, I think, sufficient to justify the definitive rejection of the first hypothesis in all its forms; for, on the one hand, we have seen that no disorder of the systemic functions or of the nervous centres which preside over them is capable of inducing a state which can be identified with febrile pyrexia; and, on the other, that it is possible for such a state to originate and persist in the organism after the influence of the central nervous system has been withdrawn from the tissues by the severance of the spinal cord. We are, therefore, at liberty to adopt the tissue origin of fever as the basis on which we hope *eventually* to construct an explanation of the process."

As variations in temperature in fever seem to measure more accurately than anything else the variations in the condition of a patient suffering from fever, the effects of various treatments on febrile temperature have lately excited special attention.

Three plans for the reduction of temperature seem to have been pursued :—

1. The natural one of application of cold (the cold bath, etc.).
2. The administration of aconite and similar agents, which reduce the force and rapidity of the circulation.
3. The administration of quinine, etc., which tends to diminish the febrile processes by counteracting the growth of contagium.

It must be admitted that the excess of heat produced during fever is the result of the chemical process going on within the body, and that these chemico-vital phenomena are the result of the growth of the contagium, which will continue until all suitable soil is exhausted. Taking this view of our question, it scarcely seems reasonable to expect that the mere abstraction of heat will in any way retard the processes going on. It is somewhat like attempting permanently to reduce the temperature of a room, over-

heated by too large a fire, by occasionally opening the windows. No sensible person would make the attempt, but at once reduce the fire. The opening of the windows would make the room more comfortable for its occupants for the time being, just as cold water would make a fever patient more comfortable, but the cause of the fever is not influenced. I have tried (as I felt bound to) the use of cold water in the treatment of fever, but must confess I found it useless in all, and even injurious in some cases.

The grounds for the employment of such drugs as aconite for the reduction of temperature seem to be insufficient. They lessen the rate of the circulation, and therefore reduce the amount of heat carried by the blood to the surface in a given time, and in consequence, the sensible amount given off; but does not this rather tend to prolong the *fever* than to diminish it? If the heat is produced by tissue change it must be given off, and aconite has not been found to shorten fevers. Aconite is a powerful depressor of the circulation, and its use on this ground alone is contra-indicated in fever.

The grounds for the use of quinine and similar drugs are much stronger, as they absolutely tend to *diminish* the febrile processes, and not merely remove the *result* of the processes. It is in the study of the antipyretic drugs of this class that the greatest triumphs may be expected. It is among these that we must look for the power of cutting short a fever, as Rheumatic Fever can now be cut short by the use of salicylic acid. The doctrine that a fever cannot be cut short must be given up by physicians, and with it, of course, the so-called "expectant treatment of disease." But another question still arises. The most dangerouss pecific contagious diseases, as a rule, attack individuals only once in their lifetime; and the accepted explanation of this is, that the first crop of the contagium exhausted the soil necessary for its growth, and until a new soil be produced the same disease will not again take root. Now, supposing we cut short the progress of a contagious fever by killing its germs within the system before the soil is exhausted, will not the soil so left render the patient liable to a future attack of the same disease? This is a question yet to be answered.

I trust I have proved, and satisfactorily so, that the state of our present knowledge of the intimate nature of contagion is sufficient to guide us in the prevention and cure of disease, and to encourage us all to earnestly prosecute its study.

OPERATION REHEARSALS ON LIVING ANIMALS.

FROM a mass of correspondence forwarded to us by Mr. James Cowie, M.R.C.V.S., of Sundridge Hall, Kent, we learn that by dint of great exertion and perseverance, much travelling and interviewing, as well as argument and persuasion, and no doubt considerable expense, he has been successful in directing the attention of the French and Italian Governments to the horrible cruelties perpetrated by students of the veterinary schools of those countries, in their rehearsal of operations on live horses. Until we had read this correspondence, we had reason to believe that the number of tuition operations each student was required to perform had been greatly limited in France—a limitation supposed to be due to representations made by England some years ago. Perhaps, however, the change to a different form of government led to the resumption of the old number of operations. Certain it is that, until Mr. Cowie's interference, they were as numerous and as horribly cruel as in former days, when we ourselves joined in the crusade against the practice. Mr. Cowie went to France and Italy as the representa-

tive of five hundred members of the veterinary profession in this country, and his reception by the friends of animals appears to have been particularly gratifying; while his success with those who have the power to interfere with the practices carried on in veterinary schools was complete. He received an assurance from these authorities that the operations, for the future, would be very few in number and of a comparatively simple and painless character. We most sincerely congratulate Mr. Cowie on the result of his noble mission. He undertook a task which was heavy in more senses than one, and accomplished it in a manner which does him great credit and honour; while he has largely benefited humanity by removing a stain that lay deep upon the reputation of a profession whose sacred mission it is to prevent or relieve suffering in animals.

QUEEN'S CADETSHIPS.

WE have often commented on the little encouragement given to Army Veterinary Surgeons by the Government, when compared with the officers of other departments of the Service, and have expressed a hope that some of the boons and privileges which have been for so many years conferred on these might be extended to our Department. It therefore gives us the greatest satisfaction to announce that one of these privileges has at last been conceded, in the form of a Queen's Cadetship bestowed on the eldest son of the late Inspecting Veterinary Surgeon, W. Partridge, who died not long ago of disease contracted in the service of his country. This valuable and most worthily-bestowed acknowledgment of professional services was not secured without much pressure and solicitation—as there was "no precedent" for it; but, thanks to the efforts and influence of the Principal Veterinary Surgeon, as well as to the high respect entertained for the deceased officer's father-in-law, Mr. Gloag, formerly Principal Veterinary Surgeon to the Army in the Crimea, the boon was ultimately granted. We trust this gracious concession to the merits and utility of the Department will soon be followed by others, and that in a short time we may see the last of the objectionable and disheartening words—"no precedent."

INTERMITTENT SCABIES IN THE LARGER HERBIVOROUS ANIMALS.

AT a meeting of the Central Medical Society of Paris, held some time ago, M. Mégnin drew attention to the fact that certain of the domesticated animals are capable of being affected with a form of Scabies (*Gale*), which presents the singular character of being intermittent: becoming fully developed in winter, and disappearing entirely during the summer, only to appear again in the course of the following winter.

This form of Scabies is caused by an *Acarus* belonging to the family of *Sarcoptidæ*. It was observed for the first time by Hering, who named it, from the species of animal on which he discovered it, the *Sarcoptes bovis*. Found on the horse by Gerlach, and about the same time on the goat by Delafond (in 1854), it became, for the first-named authority, the type of a new genera under the name of *Symbiotes;* while by Delafond, who was in ignorance of Gerlach's researches, it was named the *Sarcoptes caprea*, and afterwards the *Sarco-dermatodectes*, which furnished Gervais with the type of his genus

Chorioptes. Fürstenburg, justly remarking that the term *Symbiotes* had been already employed in entomology—among others by Redtenbacher to designate a kind of *Entomychides*—endeavoured, in 1860, to substitute for the word *Symbiotes* that of *Dermatophagus:* a name improperly bestowed through an error in observation, because all the parasitic *Sarcoptidæ*, without an exception, live by sucking the nutritive fluids of the organisms they infest—be they animal or vegetable, healthy or diseased.

Mégnin thinks it best to adopt the name given by Gervais—that of *Chorioptes*—a course which he has subsequently followed. But he asks if there are more than one kind of *Chorioptes;* for having already exactly determined and scrupulously described, under the name of *Chorioptes spathiferus*,* that which he found on the horse, he is now seeking to discover if those parasites observed on the ox by Hering, Gerlach and Fürstenburg, on the goat by Delafond, and on the dog by Lucas and Nicolet, constitute the same or different species.

In the zoological history of the *Chorioptes spathiferus*, Mégnin had stated, on the faith of his colleague M. Collignon, who had afforded him the opportunity of studying the parasite, that the disease to which it gave rise on the horse offered the singular character of disappearing completely during the summer, to return with all its violence on the approach of winter; and it was this peculiarity which induced Mégnin to state that the malady was not psoric in its nature, but constitutional, and related to the Strangles diathesis (*diathèse gourmeuse*). In Germany the same remark had been made with regard to the Symbiotic Scabies of the ox (*Magazin für die gesammte Thierheilkunde*, 1860); but the cause of this intermittence remained as obscure in Germany as it did to Mégnin in France. A new case of Chorioptic Scabies occurring in a young mare suckling a foal, at length afforded Mégnin a key to the problem. This animal was affected with the disease during the preceding winter, all the limbs below the knees and hocks being involved; and though not put under treatment of any kind, the malady disappeared spontaneously and completely when the winter's coat was shed in the spring.

Notwithstanding this disappearance of the psora, Mégnin continued his observations on the regions which had been affected, and to his astonishment found that the *Chorioptes* still occupied them, and that the parasite was as lively as during the height of the disease. The only difference appeared to be in there being fewer ova, larvæ, and nymphæ; multiplication also seemed to be arrested, or at least singularly retarded; but otherwise the adult parasites wandered at the root of the hairs exactly like the *Myobiæ* and *Listrophoræ* on the skin of the mouse, or the *Sarcoptidæ avicola* amongst the feathers of birds.

During the three months the case was under observation, there was not the faintest trace of the usual pustule or vesicle of Scabies, and the epidermic exudations were no more abundant than in health. It was therefore concluded that the *Chorioptes* lived exclusively, during this period, on the cutaneous humours, which of course were in greater quantity in the warm weather: in a word, the creature had changed its mode of life and assumed that of its neighbours—the *Dermaleichus*—becoming simply a *parasite* instead of a burrower in the skin, as it had been and as it would be again when the cutaneous secretions were diminished by the advent of the cold weather.

Here, therefore, was a horse which, for six months, carried on its limbs the germs of a disease that could scarcely be called *latent*, as a malady cannot be characterised by its cause alone; and this horse, without being actually diseased itself, was yet in a condition to transmit the germs of

* See the *Journal de l' Anatomie* of Robin, for July 1872; also "Veterinary Sanitary Science and Police," vol. ii., p. 411.

Chorioptic Scabies to any horses with which it might come in contact; these horses themselves becoming "mangy" after a time, and apparently without having been near any other diseased animal. Luckily this form of Scabies is not very contagious, the *Chorioptes* having but little inclination for a change of quarters.

Mégnin justly remarks, in concluding his notice of this case, that the transmission of certain contagious maladies is extremely interesting to study, when we can trace and follow the "animated ferments" which cause them through all their phases and peregrinations. But, unfortunately, all the so-called "ferments" are not so easily scrutinised as the *Acaridæ*.

Reviews.

THE CORN AND CATTLE PRODUCING DISTRICTS OF FRANCE. By G. G. Richardson. London: Cassell, Petter & Galpin.

Since the publication of Arthur Young's "Travels in France in 1787, 1788, and 1789," it may safely be asserted that English reading people have had no opportunity of acquiring anything like a satisfactory or complete notion of French agriculture; and indeed our neighbours across the Channel had nothing better on the same subject, for a long time, than a translation of Arthur Young's most excellent and very exhaustive work. There are many reasons why a knowledge of the agriculture of our nearest Continental neighbours should be valuable and interesting to us. There are ties which bind England strongly and closely to France and the French people. "La belle alliance" has now enjoyed an undiminished reign of many years, and we earnestly hope it will never be interrupted. Much of the world's progress depends upon the amity existing between the two countries. The great exhibition which has taken place in Paris this year is a proof of the high position France holds among the civilized nations of the globe, and she has given ample evidence that, in everything which tends to promote the well-being of mankind, she occupies a place second to none. As a country, France presents many features to interest every one. "This vast territory," writes Lavergne, in his "Rural Economy of France"—the best book on French agriculture in that language—"which reaches from the Alps to the Pyrenees, from the Mediterranean to the North Sea, this mixture of plains, hill-slopes and mountains, opened up in every direction by four great streams, watered by hundreds of rivers and thousands of rivulets, like veins in the human body,—those immense tracts of grass-land of the west coast, those noble old forests of the east district, those green pastures of the centre, those rich vineyards of Burgundy and Languedoc, those olive and orange groves of Provence, those golden harvests waving on all sides, which bear the largest crop of corn in the world, that union under the same laws of an infinite variety of climates and people, this epitome of the Low Countries and Spain, of England and Switzerland, of Germany and Italy,—this bright collection of contrarieties, is our lovely, our dear France."

Mr. Richardson has, therefore, done well to give us such an account of the corn and cattle producing districts of France, as will satisfy the most exacting of inquirers in many subjects connected with that country. To the agriculturist who cares to look beyond his native isle, the work before us offers particular attractions; and to those members of the veterinary profession who understand the close relationship that exists between their science and the science of agriculture, it is not without a high degree of interest, and especially as regards the live stock of France; while to the general reader, one of the motives for writing the work should commend itself—"a desire to make known to English people more of one side of French life, which has so many excellencies, and which, perhaps, has hardly had justice enough allotted to it by us."

There are many parts of Mr. Richardson's book to which we are tempted to give a lengthened notice, but lack of space prevents us alluding to them all. It may interest our readers to know that the number of horses in France may be taken at 3,000,000, of which it is computed that 1,080,000 are under four years old, 365,000 are fifteen years old and upwards, 352,000 are entire horses, 600,000 are brood mares, and 303,000 inefficient from accident or disease; leaving 300,000 from four to fourteen years old, and sound, of which 81,000 are in the army. Of the total number of horses (3,000,000), 1,800,000 are heavy draught animals, 600,000 to 700,000 are light, and from 400,000 to 500,000 are medium-sized horses. It is from these last two classes only that horses for the army can be taken. The army at one time required 70,000 horses,—but to fill up the regiments as re-organised after the late Franco-German war, 90,000 have been demanded. The estimates of the Minister of War for 1878 even reach to 110,000 for the army, and 13,000 for the *gendarmes* or mounted police; while the Algerian regiments absorb 16,000 of the number. The price of troop horses for 1878 is fixed at £46 for heavy cavalry horses, £40 for dragoons (or mediums), and £36 for light cavalry. Officers' horses are £10 dearer. The price of troopers' horses for Algerian regiments is £28, and officers' horses £32. In case of war from 250,000 to 260,000 additional horses would be required, of which 176,000 must be furnished at once—viz., 98,000 for heavy draught, 78,000 for cavalry and artillery. And to supply this requirement, there are only 219,000 horses of all kinds, and of a serviceable age and sound.

Richardson enters fully into the details of horse registration, which renders all the horses in France promptly available; as well as into the breeding, organization of studs, etc.; and compares the equine resources of that country with those of Prussia, Austria, Hungary and Russia. The schools for training horses, the horse-shows, etc., are also described; and the different breeds, numbers, and qualities of the cattle, sheep, and other creatures composing the animal wealth of our Gallic neighbours, occupy much attention.

Among the wild animals to be found in France (such as the bear, fox, badger, otter, etc.), it appears that the wolf is the one whose destructiveness is greatest and most dreaded—the value of the domestic animals yearly destroyed by the 2000 supposed to exist being estimated

at £80,000. Notwithstanding this destruction, the wolf is carefully protected as an animal for sport, and there are more wolves in France now than there were some years ago.

The agricultural features of the different provinces of France form a most interesting section of the book under review, and this is well worthy of perusal. The mules of Poitou are famous all over Europe, and we learn that the main destination of horses in Poitou is the breeding of mules. Out of 90,000 horses in the three departments of that province, 60,000 are mares; and of these 50,000 are so employed, 40,000 being given directly to jackass sires, and 10,000 to sires of their own race, to keep up the supply of mares and sires necessary for the trade. Not more than half of the mares put to the jackass produce foals; and taking into consideration the accidents which happen to those that are born, it is estimated that 18,000 mules are annually reared and sold in Poitou. Some of these make a high figure—as much as £60 being not uncommon; a considerable number make £40, but £24 is considered a fair average. At this estimate the large sum of nearly £450,000 is reached. This is paid to the Poitou breeders annually by outside buyers, for none of the mules of the full age of four years remain in the country: at the end of the season not a mule prepared for the market is unsold.

We are informed that the cost of rearing mules is very small. The dams are poorly fed, and from the time of weaning up to the age at which the young mule pays for his keep, only ten or twelve months elapse; there is no real outlay except for the three months during which he is prepared for market. The mule changes owners every year of his life until he is bought for service. Small farmers who do not breed buy the young mules and wean them, selling them again to other farmers who can work them; and they again sell them to dealers whose special business it is to fatten them up for sale. This preparation takes place in a close stable removed from noise; a stone tank of water provides drink always at the same temperature; abundant and good food is given, but air and light are both carefully excluded, and neither can penetrate except during the short time the stable is cleaned out daily. Under this treatment the mules soon begin to sweat, and they continue bathed in perspiration; the air is so hot and so charged with vapour, that the walls run down with moisture. A few days before the fairs, they are taken out and walked for about three-quarters of an hour daily, and they are so changed as to be hardly recognisable. Lean, overworked, dull, when first put up, they are turned out as fat as bullocks; and from being dejected, docile, spiritless animals, they become bright and cheerful. "It is difficult to believe that the dirty, forlorn brute that was so recently dragging painfully at the plough can be the same animal, now decorated with a white leather bridle and blinkers, his head reined back to a girdle, holding himself high, pawing the ground, and justifying his owner's bold demand of £40 at least, from the dealers of the south of France or Spain; or if not up to the standard of these exacting buyers, then satisfying the more moderate requirements of the purchasers for the Colonies, both French and English, or those from America." Mr. Richardson remarks: "It might be supposed that such a treatment—a change so sudden from

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semi-starvation to high feeding, and from close stabling to exposure to open air—would be likely to produce disease; but no complaints on this head are ever heard; and, what is equally surprising, the animal so full of life and energy on his way to the fair, starting and snorting at every new object on the road, becomes quiet and calm when he joins some thousand or so of his companions, and allows the buyers to pass close to him, to examine his points, without a kick from any one of the thousand."

The information as to the Poitou mares and jackasses is almost equally interesting and curious to that regarding their produce; but we cannot dwell longer on Mr. Richardson's really valuable work. It is a mine of information on the subject of which it treats, and must be looked upon as a standard authority on French agriculture.

ANATOMICAL OUTLINES OF THE HORSE. By J. A. McBride, Ph.D., M.R.C.V.S. (Second Edition, revised and enlarged. By T. Walton Mayer, F.R.C.V.S.) Cirencester: Baily and Son, 1878.

It is somewhat strange how persistently veterinary writers on anatomy in this country cling to anatomical outlines of the horse, and neglect the anatomy of other animals whose structure is, in some respects, equally useful to be known by the student and practitioner. This almost unpardonable neglect may be due to the fact that the anatomy of the horse or ass, as convenience might require, was alone taught at the schools, and that whole generations of students have passed through their portals after a brief period of study, without seeing any other animal dissected, or hearing anything of the differences in structure of these. Strangeways, in utilising Chauveau's Anatomy, certainly departed somewhat from the rigid rule, and briefly noticed structural differences in several of the domesticated animals, his book being illustrated with photo-zincographs from the German work, Leyh's Anatomy. One would have imagined that a book on anatomy intended for the use of agricultural students, would have had some reference at least to the structure of one or more of the other useful creatures than the animal which receives so much worship among Englishmen. But such is not the case in McBride's handy little Outlines of the Horse's Anatomy, and a new edition of which, by Mr. Mayer, Veterinary Professor at the Cirencester Agricultural College, has just been issued.

As an elementary book for agricultural students, or a kind of *aide mémoire* for those of veterinary schools, the "Outlines" appears to be well adapted; and we have no doubt the editor has carefully revised it, and made those additions to it of which his experience suggested the desirability. It is well printed, and the descriptions are simple and clear; besides, it is provided with eight familiar illustrations, six of which are coloured, and add to the value of the book. In concluding the preface, the editor offers his acknowledgments to the following authors "who have already contributed to the advancement of physiological and anatomical science—viz., Dr. Mapother, Charles Steel, 16th Lancers, and John Henry Steel, Demonstrator of Anatomy at the Royal Veterinary College, whose works contain much valuable informa-

tion." These acknowledgments do the editor of the "Outlines" credit; but he has evidently forgotten the author of the excellent drawings which illustrate the work—M. Mégnin, our able and accomplished colleague in the French army—whose veterinary diagrams (of which the above are a portion) are now sold in this country, and the translation of the descriptions for which we ourselves prepared for Mr. Kimpton, of High Holborn, three or four years ago. The translation is that which appears in the "Outlines." Our French colleagues are rather tender upon these omissions; and as Mégnin has also done something to advance anatomical and physiological knowledge, he may think he is likewise entitled to honourable mention when a portion of his artistic work is deemed worthy of being transferred to an English book like the present.

Proceedings of Veterinary Medical Societies, etc.

SCOTTISH METROPOLITAN VETERINARY MEDICAL ASSOCIATION.

THE quarterly meeting of the above Association was held on Wednesday, the 11th September, in the London Hotel, Edinburgh. Professor Baird, of the Dick Veterinary College, occupied the presidential chair. There was a good attendance of members. Mr. Spreull, F.R.C.V.S., Dundee, and Mr. A. Baird, M.R.C.V.S., Edinburgh, were elected members of the Association.

Professor WALLEY again called attention to the desirability of members joining the Mutual Defence and Benevolent Association, and moved that the Secretary inquire what steps had been taken to bring the matter under the notice of the members.

The motion was seconded by Mr. ROBERTSON, F.R.C.V.S., Kelso, and carried.

The following resolution, moved by Professor WALLEY and seconded by Mr. ROBERTSON, was carried unanimously:—"That the Secretary be instructed to issue circulars calling the attention of the members of the profession in Scotland to the movement at present on foot to present Mr. George Fleming with a testimonial, and that he be empowered to solicit and receive subscriptions towards a fund for that purpose."

Several interesting pathological specimens were brought for the inspection of members by Professor Walley and Mr. Storie, M.R.C.V.S., East Linton. The following is the history of one of these, as related by Professor Walley:—

"*Case of Abscess in the Right Arytænoid Cartilage.*

"On the night of the 29th of May, my attention was directed to a valuable dark-brown lorry horse (seven years old), which was said by his driver to have gradually become a roarer. The driver had noticed a roaring sound in the breathing of the animal, when at work, for several weeks, and had spoken of it to the horse manager, who treated the matter lightly. On examination I found that a distinct roaring sound was heard, even while the animal was quiescent in the stall: it was much increased by excitement. External manipulation of the larynx caused violent coughing and uneasiness. Both externally and by manipulation through the mouth, I detected hardening and enlargement of the larynx. The animal was in perfect bodily health

and condition. I contented myself at the time with the application of ungt. hydrarg. iodid. to the region of the larynx, ordered him to be put into a loose box, and his diet to be carefully regulated. The following day the difficulty in breathing became so great as to necessitate the operation of tracheotomy, which I had—owing to the great swelling caused by the blister—to perform at the lower part of the neck. Immediate relief followed the operation, and for several weeks all appeared to be going on favourably, with the exception that more thickening of the tissues around the seat of operation took place than usual. In about three weeks a large quantity of pus began to find its way through the canula, and although it gradually decreased in quantity, it became more adhesive, and contracted a sickly odour, with all the physical properties of fistulous discharge. It frequently plugged up the canula. The tissues in the seat of operation became considerably thickened and indurated. About a month after the operation the horse was sent to work in traces, but it was found that a very little exercise was sufficient to produce great distress, excessive perspiration, and loud roaring. This led me to suppose that some obstruction existed below the seat of operation. Physical examination of the thorax, with exploration of the trachea by the aid of a male catheter, failed, however, to satisfy me on this point. A mercurial blister was applied over the larynx, and to the anterior pectoral region, iodine administered internally, and subsequently setons inserted on either side of the larynx. All treatment was of no avail; and in four or five weeks after the operation, repeated spasmodic attacks of dyspnœa (sometimes twice during the night) came on, threatening suffocation. Each attack lasted about five or ten minutes, and during its continuance the animal evinced the greatest signs of distress—the tongue becoming pendulous and livid, and the mouth widely open. It is a curious fact that the removal of the tracheotomy tube, the passing of the finger through the aperture in the trachea, and the re-insertion of the tube, nearly always caused these symptoms to pass away. Seeing the unusual nature of the case, I submitted it to the examination of my colleague, Mr. Baird, and subsequently to Professor M'Call, both of whom coincided with me in the opinion that, unless some mechanical obstruction existed in the lower part of the trachea, the paroxysmal attacks of dyspnœa must have a nervous origin. If the animal was hurried for a distance of ten yards at this time, a dyspnœic attack was induced. About 7 p.m. on the 12th August, the horse died, after a few minutes of intense suffering. I had cast him the previous day, and made a careful exploration of the larynx through the mouth, without discovering anything more than thickening of the right side of the larynx. *Autopsy.*—Body in splendid condition; throughout the illness the appetite remained good, and there was no evidence of wasting. All the abdominal organs healthy. *Lungs.*—Congested at their upper borders. *Heart.*—Considerable dilatation and thinning of right ventricle, otherwise normal. *Trachea and bronchial tubes.*—Except at the seat of operation, where signs of irritation in the mucous membrane existed, healthy. *Larynx.*—On opening this organ, a fistulous opening, about the size of a crow's quill—through which fistulous pus oozed on pressure—was discovered at the upper and inner part of the right arytenoid cartilage, which was bulged and considerably thickened. Section through the cartilage exposed an old cyst in its very centre, its walls being thick and vascular, and containing pus. Unfortunately it did not occur to me, until too late, to make an examination of the pneumogastric nerve. The conclusions at which I have arrived are, that from some undefined cause an abscess had formed in the centre of the arytenoid cartilage, in due course evacuation of its contents had taken place into the interior of the larynx, a secretory pyogenic action in the sac had continued, a permanent fistulous orifice was established, and the pus had insensibly flowed down the trachea and been expelled through the canula.

I have not the slightest doubt that the paroxysmal attacks of dyspnœa were to a great extent of a nervous origin. The cause of the pulmonary congestion (though its seat, the upper borders of the lungs, was unusual) and the cardiac dilatation is manifest."

Mr. ROBERTSON thereafter read a paper on the "Morbid Anatomy and Pathology of Arthritic or Joint Disease of Young Stock."

"My object in introducing for our consideration to-day the subject of Arthritic or Joint Disease in young stock, is not so much that I may publish or give expression to any particular opinions of my own regarding its nature or causes, etc., as that there may be afforded to all an opportunity of giving a plain statement of experience, not an expression of opinion,—a statement from my professional brethren as to those conditions under which in actual practice they have met with this malady. For we may depend upon this, that it is only by a careful recording of facts and phenomena as they exist and occur, that a safe foundation is to be laid for any advances in the science of pathology. We ought ever to bear in mind that medicine is much more a science of *induction* than of *deduction;* that it will not do to construct theories of disease and endeavour to shape facts to suit these; that, rather, must we start from facts; and that a careful and faithful record of these is likely to be of more use in the study of disease than the framing of any hypothesis, however ingenious or elaborate. This Arthritic Disease of young stock, although for ages known and recognised by those connected with the breeding of animals, has not, in this country at least, received from scientific veterinarians that amount of attention which its importance demands, while wherever it has received more than a merely passing attention, either from ordinary observers or from those more specially qualified to interpret its phenomena, the opinions regarding its nature and causes have already been exactly alike. Causes *endogenous* as well as *exogenous*, and these very dissimilar in their character, have alike been credited with its production. It has been looked upon as a truly constitutional and hereditary disease: as originating from purely external causes, operating entirely in a local manner; or as essentially a dietetic disorder, where the errors of dieting have begun to operate on the young, not at birth, but during intra-uterine life. That such different opinions come to be entertained regarding its origin is not difficult to understand, when we find that there is a certain difficulty in laying the whole onus of its production on one isolated condition, and when apparently many and varied influences are operating in its production, or intimately associated with it. It is characterised by uncertainty of appearance, both as respects seasons and locality; it is rather obscure in its etiology, and decidedly destructive in its results. Breeds do not seem to confer an immunity from its attacks, although, as the result of my experience, I am free to confess that what are regarded as the more highly-bred and improved animals—those upon which most attention as regards their breeding and feeding has been bestowed—seem more subject to its ravages than others differently operated upon. It is essentially a disease of early life; rarely if ever, amongst horses and sheep at least, appearing after the period of lactation; most common until the second month of their age—and more fatal in the younger animals. It does appear sporadically, but is most frequently introduced to our notice as an enzoötic, and is then most characteristic in features and destructive in its results. Not confined to our own country, it is well known for its speciality of character and untoward terminations over the greater portion of continental Europe; and has everywhere, at irregular periods, caused stock-owners much anxiety by its more extensive distribution, or more generally fatal results. If not the very chief ailment of our foals, calves, and lambs, it certainly holds in the list of diseases a position of marked prominence. Its fatality is always great—ranging from 30 to 70 per

cent.—while of those which do not succumb, the recovery is seldom so complete as to render them remunerative either as feeding or breeding animals. Although occurring in all the three species of our domestic animals—the *equine*, *bovine*, and *ovine*—it is probably best known, because most extensively distributed and appearing in its most malignant form, in sheep; while, like too many of the diseases attacking these animals, it has been quietly submitted to as if disease here belonged to the inevitable. The enhanced value, however, of these animals as sources of the food supply of our population, and the steadily increasing value of animal food, an absolute necessity in the dietary of our hard-working people, has undoubtedly been the means of directing more attention to the conservation of the health of these creatures. Although the phenomena connected with the appearance and development of this specific Arthritic Disease in horses, cattle, and sheep, the animals in which it is ordinarily met with, are marked by certain special peculiarities in each species, there is yet enough common to each to lead us to regard it as essentially the same affection in all these, and as most probably owing its existence in each to the same or similar agencies. As far as my experience goes, I am satisfied that with the young of the equine species it is more steady and regular in its occurrence year by year, and that with them it more rarely attains such an extensive distribution and truly enzoötic character as with lambs; in both, however, the fatality is excessive. With sheep there are certain seasons when extremely little of this disease is seen; others, again, when its enzoötic character is clearly established. It is also worthy of remark that, coincident with and accompanying these enzoötic outbreaks, we meet with the appearance of certain peculiar blood disorders, both amongst the breeding ewes and lambs. Whatever has been or may be the relative susceptibility in other countries of the different species of domestic animals to this disease, I am rather inclined to believe that not until the last quarter of a century has our experience of it in this country entitled us to regard it as at all a common disease of the equine race; while even within this period it seems with these rather to show a tendency to increase. This does not seem to be the case with either cattle or sheep. Over continental Europe it has secured a place in veterinary literature, and attracted attention as a serious disease in the young of all our principal domestic animals for at least a century. I do not, Mr. Chairman and Gentlemen, intend to enter into and describe in detail the symptoms of this affection in either its first or last stage, as seen in the various species of our patients. I reserve this for another and different occasion. What I now wish to direct your attention to, and if possible elicit some statement of experience or experiment regarding, is rather in the direction of its pathology, and of those conditions and agencies which apparently have a bearing upon its etiology. There is amongst all of us a wonderful uniformity of opinion as to the diagnosis of this disease. It is rarely confounded with aught else. We are also at one as to the period of life when it is most likely to occur; while the opinion that the mortality is in all cases great, is generally accepted as correct. Beyond these matters there is, however, very little agreement. The nature of the affection, and the causes which tend to its production, have furnished material for much difference of opinion, both amongst stock-owners and shepherds, and comparative pathologists. With the former class it has in a general way been regarded as local inflammation of the parts most visibly affected; while, in looking for causes, rarely have they passed beyond the external and local influences associated directly with atmospheric conditions, or character of soil, as to its comparative dampness, etc. Any very extensive and more than usually distributed or severe outbreak has at once been attributed to some meteorological disturbance, or to the influence of contagion. Those

of us, again, who have aspired to a little more special knowledge, have for a long period, to put it in the mildest words, been hesitating and uncertain as to our views of the pathology of this serious disease. There has been little definiteness or satisfaction as regards the conclusions arrived at; and opinion has wavered and been divided between these different views of its nature. To one or other of these, the most of us who have given any attention to the subject have at some time sworn allegiance. It has been regarded as a constitutional disease in which, according to the species invaded, either the rheumatic or scrofulous diathesis was in the ascendant. Another view of its pathology, and one largely supported by facts and advocated by the most celebrated comparative pathologists of our time, is that which regards it as truly septic or pyæmic in its origin—the inoculation proceeding from purulent inflammation of the umbilical cord. Another class of observers is inclined to view this affection neither as constitutional nor as primarily septic, but rather as essentially dietetic—a systemic disturbance which, although evidencing itself by certain local symptoms or phenomena, notably in connexion with the articulations, is nevertheless essentially a disorder of nutrition traceable to causes local and preventable. Let us endeavour to see how far these different theories are in accordance with the facts and occurrences which all of us have encountered. There is no doubt that isolated cases amongst all species will present themselves, which can only be understood by taking into consideration the special surrounding and influencing agencies; but this is not what is wanted, nor what we desire. It is rather to see if the majority of cases, if the great developments or outbreaks of this disease, possess any certain diagnostic characters in common which shall naturally allow it to fall under one of the ordinary accepted views, or in any way enable us to comprehend the causes in operation for its production. *Pathological Anatomy.*—With this object in view, we shall first of all glance shortly at the anatomy of the disease as illustrating the deviations and alterations from the usually accepted standard of health, and some of the tissue changes resulting from the morbid processes. The appearances of disease, as exhibited in examination after death, are not confined to the localities ordinarily regarded as the seat of the diseased action—the joints; but may very justly, as Professor Walley has said, be spoken of as systemic and local. In foals dying from this Joint Disease, there is in the greater number the strikingly abnormal condition of pervious urachus. During life the animal may have voided urine both by the natural channel and the umbilical opening. Examination after death in all these cases discloses an extremely unnatural condition of all the structures entering into the formation of the umbilical cord. There is in the condition of all these the same characteristic features—a want of perfectly developed and carried out formative action, or the beginning of direct degenerative changes. The urachus, besides its unclosed condition, has its lining membrane roughened, of a dirty grey colour, studded in places with a puriform, varying coloured, aplastic material. The umbilical vein is usually the most markedly altered of the blood conduits; it is much distended, the walls thickened, and its interior having somewhat the same appearance as the lining membrane of the urachus, and spotted in like manner with a greyish-white puriform deposit. The connective tissue peculiar to the cord holding its different constituent parts together is much changed, and in all those cases where omphalitis has existed, and also in several where such has not been present, we find extending from the umbilical ring to the vena porta and vena cava a chain of cysts or abscesses. Of the internal organs, the liver is that which most regularly exhibits distinct structural lesions. Much increased in size from distension with blood, its colour is usually paler; while under the peritoneal covering over parts are seen well-marked blood spots,

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and in its intimate structure circumscribed collections of pus, surrounding which the gland structures appear paler, as if undergoing some fatty change. In many instances the organs of the chest are involved, the serous membranes of the heart being marked more or less distinctly with petechial spots or blood extravasations; while the lungs give evidence of catarrhal bronchopneumonia, and occasionally embolism of some of the pulmonary blood-vessels is a marked feature. The glandular organs, spleen, lymphatic and mesenteric glands, with the serous envelopes of the abdominal cavity, will sometimes show alteration of structure hyperplastic in nature, with infiltration into their constituent connective tissue; with the existence in the abdominal cavity of much turbid fluid, having floating in it shreds of degenerating lymph. In addition to these pathological changes in the internal viscera, there is also met with amongst the muscles of locomotion, markedly in the gluteal regions, extensive effusions of lymph or serous fluid, mingled with pus and puriform materials. In the blood itself, the formed materials, the corpuscular elements, are evidently alterated, the colourless cells augmented in number, and the others breaking up and undergoing retrogressive changes. With the exception of the open urachus, which is more particularly confined to solipeds, these general structural changes are encountered in all species of our patients—sufferers from this Arthritic Disease. The local structural changes—those confined to the joints—are not restricted to any one tissue entering into the formation of these parts, but are met with in all. Variations undoubtedly exist as to the extent to which these structures may be severally invaded, and being formed of somewhat differing elemental tissues, they must of necessity comport themselves somewhat differently under the same diseased action. In cases of recent origin we may see little save infiltration of the tissues external to the articulation affected, with evidence of inflammatory action in the synovial membrane; the majority of those cases, however, where an autopsy is carried out, having existed for some time, exhibit more marked changes of structure, membranes are thickened, softer, and less clear. The capsule of the joint is distended with an extra quantity of a dark-coloured fluid, having floating in it detached shreds of softening and degenerated lymph. The cartilage covering the articular extremities of the bones is discoloured in patches, and roughened or eroded, to which is adherent patches of the same partly-organized lymph, mixed with purulent material. The condition of the long bones entering into the formation of the articulation, deserve special consideration. I have, in the great majority of the cases of Joint-ill, in all species of animals found the intimate structure of these visibly altered at their extremities. There is thickening of the periosteum, a partial separation of this from its connection with the bone, an opening out or rarefaction of the tubular tissue of the bones themselves, and a distinct bulging of their extremities, the cancellous texture being filled with a yellowish or red-coloured jelly, or sometimes with a paler, cheesy material. The cartilages of development between the epiphyses and shaft, show in all these cases a marked alteration, softening of texture from degeneration of the matrix and increase of cell elements. This alteration of bone structure, so much resembling rachitism, does not occur as a sequel to the inflammatory condition of the other joint structures, as merely a further development and extension of the process, but is seen in very many instances at the very outset of the disease. I am aware that this has been left unnoticed, or rather, I ought to say, has been noticed purposely to take exception to its occurring either regularly or on an extensive scale, by that most scientific of pathologists, Bollinger, of Munich; but I mention it, as in my examinations it has repeatedly and for years struck me as characteristic, and in this view I am supported by many others. Roloff, who has paid much attention to the subject, speaks of it as the most diagnostic feature of the disease. *Nature or Character of the*

Disease.—The general or constitutional fever and disturbance connected with Joint Disease, together with the local symptoms of disease and certain tissue changes, have some resemblance to Rheumatism in its ordinary acute form, and as such it has by many been so regarded. No doubt, in most of our patients Rheumatism is met with; and it is perfectly intelligible that the rheumatic diathesis may be developed coincidently with this or any other articular disease. Still there are many reasons preventing us from giving full acquiescence to the statement that this Arthritic Disease of young stock is simple Acute Rheumatism. The general fever of this Joint Disease is certainly less of a sthenic character, and the results of the local morbid action peculiar to it are more fertile of tissue change and destruction than in true Rheumatism. The tissues usually primarily involved in the one disease are scarcely those so attacked in the other. In Rheumatism, the fibrous structures surrounding the articulation are, as a rule, the first and chiefly affected; in the Arthritic Disease we are speaking of, the first onset of disease is nearly as often seen in the interior and more intimate joint textures, not unfrequently in the bones themselves—in the cancellated tissue of the extremities of the long bones entering into the formation of the articulations. In ordinary rheumatic disease the inflammatory action is more liable to shift its situation from one articulation to another, and less given to result in the formation of pus, than in this; the results of the local diseased action in the former rather tending to become organized, while in the latter they are more given to degenerative changes. Again, the causes of Rheumatism, although they may be considered as truly constitutional, are, nevertheless, more dependent for their development on conditions of localization and appreciable external influences than this other. It is, moreover, more amenable to treatment. That this disease is truly scrofulous, I am at the present time less inclined to believe than I was some years since. Scrofula, in any form, as Professor Walley has aptly remarked, can hardly be regarded as familiar to us in an enzoötic form, which this Joint Disease is. Neither do we find here, as we do in that condition, deposits or collections of new-formed and adventitious material, the so-called tubercular matter in the lungs or lymphatic glands,—situations extremely prone to exhibit such changes in scrofulous subjects. There is, no doubt, in the minute tissue of the bones, and in and around the articulations themselves, the presence of a new-formed material, somewhat like the matter of tubercle; but I have grave doubts regarding the identity of these two morbid products. The constitutional fever attendant on this Arthritic Disease is also somewhat different from that associated with Scrofulous Arthritis. In this condition it is more pronounced, reaches more rapidly, and maintains more steadily without exacerbations, a high temperature. Another difference between these two affections is to be found in the subjects attacked. In the one case, that of Joint-ill, they are all of one class—the very young; in the other there is no such restriction. Again, I am tolerably certain that, by a particular dietetic treatment of breeding animals, we can ensure a very large percentage of this disease in the offspring. The same cannot be said of Scrofula. Another very well supported view of the pathology of this Joint Disease, and one which lately has been gaining ground amongst the Continental comparative pathologists, is that which regards it as purely *pyæmic* in character. The chief exponent of this idea is Bollinger, of Munich. In referring to what this distinguished veterinarian says regarding this disease, Fleming, in his 'Veterinary Obstetrics,' says that 'this septic or pyæmic nature of the disease has not, to my knowledge, been pointed out, or at least insisted on, in this country.' Now, if you refer to Gamgee's 'Domestic Animals,' vol. ii., p. 326, you will find it referred to under the name of Pyæmia Agnorum; this, I believe, being the name given to it by me in

communications during 1863. Unfortunately, in treating of the nature and symptoms in the pages which follow, the description has got mixed up with what is spoken of as the Arthritic Disease of lambs. At that time I entertained the idea that the *septic* or *pyæmic* character of the disease, in conjunction with the existence of a peculiar *cachexia*, were the points which alone explained all the phenomena exhibited during its development and progress; now, however, I feel disposed to doubt this. In the opinion of the pathologist just mentioned, as one of the chief advocates of its septic nature, the varied phenomena and complications are believed to result from metastatic *pyæmia*, which again originates from purulent *omphalo phlebitis*. Now, it cannot be for a moment doubted that *metastatic pyæmia* is a chief feature of very many—probably of the greater number—of the cases of Joint Disease both in lambs and foals, as also that in almost all these cases this *pyæmic* state is preceded or accompanied by *omphalitis;* but that the prior existence of *omphalitis* is absolutely needful ere we can have a development of the other, and more ordinarily recognised, characters of Joint Disease, I must be allowed to doubt. May not the *pyæmic* state and the existence of multiple abscesses have a different origin, and bear a different explanation than that of regarding them as a *sequela* to a purulent umbilical cord, even where this condition of *omphalitis* and umbilical *thrombosis* exists? In every extensive outbreak of this disease, and very generally as a feature where its enzoötic character is less strongly marked, cases are met with where all the features of constitutional disturbance—with the local symptoms of pain, swelling of joints, and the existence of abscesses amongst the muscles—are observed; while the examination after death shows the usual and varied structural changes within and around the articulations, together with the presence of multiple abscesses in the liver and spleen, hyperplasia of lymphatic glands, and tissue changes in the great serous membranes, without any well-marked inflammatory condition of the structures of the umbilical cord. While, again, I have frequently encountered in calves very serious cases of purulent omphalitis, unattended with joint complications; and when these cases have terminated fatally, I have more often, in *post-mortem* examinations, found numerous collections of pus (particularly in the liver) alone, than in combination with complicated Arthritic Disease. In regarding this disease as simply *pyæmia* or *septicæmia*, resulting from purulent *omphalo phlebitis*, we have a numerous and well-arranged series of facts and observations to support us; still it seems to us at least as rational a conclusion, and one open to fewer objections, and capable of explaining as satisfactorily the many complications and phenomena, to regard it as a constitutional dietetic disease, having its origin during intra-uterine life,—the varied tissue changes, etc., met with in the subjects affected being but the natural result of the effort put forth by the system to form healthy tissue from ill-elaborated and imperfectly formed blood. We are the more inclined to this view from the following considerations:—1st. That in very many cases of the disease there exist those most obvious and characteristic systemic organic lesions, disseminated abscesses in the umbilical cord and structure of the liver, etc., not only very shortly after birth, but during the latter period of intra-uterine life. If *omphalo phlebitis* in these cases precedes the development of pyæmic abscesses and infiltrations, it cannot certainly be the result of inoculation with septic matter from sources without the animal itself, the manner in which this poison is said usually to be implanted. 2nd. In all well-established enzoötic outbreaks of this Joint Disease in sheep, the lambs are not the only sufferers, the ewes being also seriously affected with varied systemic or blood disorders,—Walley says malignant parturient fever. I have experienced both this and, probably to a greater extent, widespread *anæmia;* the general *malaise* in both mother and progeny pointing

to some well-established condition of mal-nutrition. 3rd. The anatomy of this diseased condition. Witness the imperfectly closed umbilical ring and pervious urachus of foals; and the rarefaction and chemically defective condition of the bones of all sufferers, seem to indicate a want of formative power and tissue development in the fœtus. 4th. That by a certain dietetic treatment of ewes a very large percentage of cases of Joint-ill may be produced at will; as also that when this treatment is altered or reversed with the same animals next season, the condition of matters is changed, to be again reproduced on a repetition of former treatment. 5th. That it frequently takes the form of an enzoötic amongst our ruminants, but rarely with the solipeds. If we are correct in regarding this as a disease of mal-nutrition—a disease resulting from the attempt of the system to form healthy tissue from ill-elaborated and imperfectly formed blood, the result primarily of faulty dietary—we have not much difficulty in understanding that with foals there is the probability that this will never reach so thoroughly an established enzoötic character as it may do with lambs or even calves. The faulty dietary of our ruminants, particular of our sheep, when such exists, is more likely to take the character of extensive localization than that of our horses. Cattle, and more particularly sheep, are more largely indebted for their food supplies to what are more properly regarded as the ordinary and natural productions of the places where they are located; and should any causes be in operation inducing a deterioration of the healthy and necessary elements of these, such adverse conditions are more likely to operate prejudicially in these animals than in others more artificially circumstanced—that is, less dependent upon such natural products in the matter of diet."

It was agreed that the subject of Mr. Robertson's paper should serve for discussion at the next quarterly meeting.

The proceedings were brought to a close with a vote of thanks to Professor Walley and Mr. Storie for their trouble in introducing specimens, and to Mr. Robertson for his paper.

J. McFADYEAN, *Secretary.*

Obituary.

THE death is announced of Frederick Bailey, M.R.C.V.S., of Leicester, on August 25th, at the age of forty-four years. Mr. Bailey was for some years in the army, and served in the Ninth Royal Lancers during the Indian Mutiny; he received the medal and clasp for that campaign. At the time of his death he was veterinary surgeon in the Leicestershire Yeomanry. He graduated in 1859.

The decease is likewise reported of William Dunn, M.R.C.V.S., of Tarporley, Cheshire, who graduated in 1840. Also of Richard Brotherton, of Australia, a graduate of 1854.

Army Veterinary Department.

Gazette, Sept. 13.

THE following Officer has been permitted to commute his retired allowance:—Veterinary Surgeon of the First Class Hicks Withers, half-pay, late 10th Hussars.

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Notes and News.

SWINE PLAGUE.—It is reported that in the southern provinces of Holland an ill-defined form of destructive disease is so prevalent among the swine, that many farmers are rapidly selling off their whole stock as a precautionary measure. The consequent glut in the market has caused a very heavy fall in prices. In the Marienwerder lowland pastures, the so-called "Hog-cholera," in a virulent form, is so prevalent that on many farms not a single swine has survived the disease.

DISEASE AMONG MULES IN GREECE.—A correspondent of the *Field* writes: "A very severe disease is raging in some parts of Greece among the mules and cattle. A swelling shows itself, more generally on the chest; and frequently in twelve or fourteen hours the animal dies, or it may linger two days. On opening the animal no cause can be found: true, the examination was made by a non-professional man. In a village in Eubœa all the mules have died. Can any of your readers suggest a remedy?" The readers of the *Veterinary Journal* will recognise in this malady Anthrax with localisation.

THE DISEASE AMONG DEER IN GERMANY.— The disease among the deer and wild boars in the Forstenrieder and Grünwalder perserves, to which we called attention in our last, is said to be now on the decline; but in the Auzingen district it continues to spread, and has been communicated, as we before mentioned, to horses, oxen, and domesticated swine. No Anthrax *Bacteria*, it appears, can be discovered in the blood of the affected animals on microscopical examination; and it is chiefly by this absence, and by the complete failure of all medicinal treatment, that the present disease is distinguished from ordinary Anthrax, with which it was formerly confounded. Whatever its exact nature may be, the disease is readily communicated to healthy domesticated ruminants by inoculation, and from these again it has been communicated to horses, speedy death being in all cases the result. It will be remembered that we mentioned that Professor Duguid had recognised the disease affecting the deer in this country as Anthrax, and had successfully inoculated guineapigs with their blood. This test, the *post-mortem* appearances, and the presence of the *Bacillus anthracis*, leave no doubt as to the nature of the malady.

FORAGE BISCUITS.—We have already drawn attention to the feeding of army and other horses with forage biscuits, and mentioned the trial we had made with a quantity sent to us for that purpose by Spratt. We now learn, from the *Army and Navy Gazette* of September 7th, that interesting practical experiments are being made in the French Cavalry with these biscuits. The trials are carried on in twelve Cavalry regiments, besides six Artillery regiments and six Train squadrons. In each of those arms the experiments are made by a squadron, battery, or company, and are superintended by a committee of the regiment, consisting of a Chef d'Escadron, a first-class captain, a lieutenant, and a veterinary surgeon. The horses, before being subjected to this diet, are weighed and measured, their general state is taken down, as well as their sanitary antecedents and their strength. In certain regiments of each arm, only the ration of oats is supplied by that of forage biscuit; in others the horses are fed entirely on biscuit, only straw for litter being issued. The full ration of biscuits consists of thirty-two or twenty-six, according to the arm. Each biscuit weighs sixty grammes—not quite two ounces. The horses during ten days are gradually used to the new food, and are then fed for another eight days exclusively on biscuit. During the trials, the weight, general state, strength, work done, sickness and other events are noted. A report of the experiments is to be submitted to the Minister of War.

RUSSIAN LOSSES DURING THE LATE WAR.—The losses among the veterinary staff of the Russian army since the commencement of the late war in Turkey, have been five deaths and thirty sick. Whether this includes the campaign in Armenia we are not aware, but we know that during a cavalry engagement in that country one veterinary surgeon was killed. Of the medical staff there were 692 doctors sick, 94 died, and 4 were wounded; of assistants 1915 were sick, 217 died, 28 were wounded, and 13 were killed; while of apothecaries 48 were sick and 5 died. A Lancer regiment which recently embarked at San Stefano, on return to Russia, lost 100 horses from the extreme heat between decks. The Russian losses in animals must have been immense, and the terrible injury her invading hordes have committed, by introducing the Cattle Plague, will long be felt in and far beyond the invaded districts.

TRICHINOUS PORK.—The Provincial Council of Silesia has determined to extend rigorously the official inspection of all fresh or cured pork offered for sale or consumption to every part of the province. Every one killing a pig, or having one killed for him, is bound to have the meat microscopically examined by an official inspector, and may neither use nor sell it, nor permit it to be used or sold, unless furnished with a certificate of its freedom from disease, nor unless each joint of the carcass is branded with an official stamp bearing letters and numbers corresponding with those on the face of the certificate. The fee payable for such microscopic examination is fixed at one shilling for each pig.

PRESENT VALUE OF HUNTERS.—Our readers may be interested to know the prices given by leading dealers for high-class horses for the chase. With risk and travelling expenses, a really first-class hunter must demand a very high figure by the time he is permanently quartered in his future owner's establishment:—"HOWDEN.—Additional arrivals by special trains for the horse fair have caused the town to present a more animated appearance, and dealers were exceedingly busy buying up all that was really good and marketable. Some capital hunters changed hands at from £300 to £400, whilst inferior animals were obtained at from £65 to £100; coaching horses are quickly disposed of at from £70 to £100."

MOSQUITOES AND THE FILARIA SANGUINOLENTA.—The *British Medical Journal*, remarks that the mosquito has been regarded as a particular nuisance by man wherever they have met. The bite of this peculiarly objectionable insect has an irritating effect upon most people, over and above the mere discomfort produced thereby. But it would appear that mental disturbance and systematic bleeding are not the only injurious actions which the mosquito can and does exercise upon us. From what was recently said at the Pathological Society, it would appear that the mosquito is the means of spreading the *filaria sanguinolenta* amongst human beings. They suck up this tiny entozoön with the blood of their human victims; and the female mosquitoes, after their banquet, go to the rivers to deposit their eggs. In doing so, the filariæ and their larvæ find their way into the water, and again to other human beings who drink it. In order to ascertain how far the mosquito is thus a carrying agent of disease from man to man, a patient known to have the filaria in his blood was placed alone in a chamber, and an opportunity furnished to mosquitoes to feed upon him. After that the spoilers were captured and dissected, and in the stomach of one no fewer than one hundred and twenty filariæ were found. This newly-found relation of the mosquito to man, adds materially to its objectionableness; for not only does it feed upon us, but it helps to spread among us an entozoön which is known to cause a definite series of unpleasant pathological consequences by its presence in the blood.

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Correspondence, etc.

MEAT INSPECTORS.

To the Editor of the VETERINARY JOURNAL.

"DEATH FROM DISEASED MEAT.—A case showing the great danger to which all are exposed who unfortunately come in contact with diseased meat, has recently occurred at North Shields. Peter Young, a cow-keeper, had the carcase of a diseased cow dressed, as if for sale, by a butcher named Walton. It appears that Walton had a slight cut in his hand, and next day felt much pain in his arm, which was considerably swollen. Two medical men were called in, and both pronounced it a hopeless case of blood-poisoning. The man died within a short time in great agony. Young was summoned before the North Shields magistrates on the 14th, and charged by Mr. McQueen, sanitary inspector, with having the carcase of a diseased cow on his premises dressed as if for sale as human food. A penalty of £5 and costs was inflicted, which many may consider hardly commensurate with the enormity of the offence, or the further mischief which might have ensued to the public therefrom."—*Sanitary Record.*

SIR,—Can anything be more conclusive, touching the value of your remarks both in your *Journal* and works on Sanitary Science, on the above important subject? How many cases occur, and are brought to the light of day, of an exactly similar character! Country practitioners could speak of scores of cases which, as soon as reported "doubtful," are handed over to the village cagmag butcher, to be dressed and sent to garrison towns. They dare not remonstrate, for their very bread is at stake if they attempt to stay this traffic. But supposing such a system to be adopted as so wisely suggested by you, all attempt at such traffic would be a thing of the past.

I beg to be, sir, yours truly,

London, *September 5th*, 1878. R. WARD, F.R.C.V.S.

THE TREATMENT OF "QUITTOR."

To the Editor of the VETERINARY JOURNAL.

DEAR SIR,—On perusing the paper by Professor Walley, read at the Liverpool Veterinary Medical Association on the 22nd July last, on the treatment of that unwelcome visitor "Quittor," permit me to say that I entirely agree with him as to the actual cautery being the only effectual remedy for it. I have had several bad cases placed under my care in the past twenty-three years, and have not failed in one single instance to effect a cure through its use. On the 5th July, 1877, I was informed of two young valuable cart-mares, the property of Messrs. Morris and Stevens, millers and corn-factors, Borough, London, which were to be sold on account of their suffering from the aforesaid disease, having been pronounced incurable by two of my professional brethren. I ran up to London by rail on the same day, and after examining the condition of the feet, I purchased the grey and black mares for the sum of £30. On the following day I sent a low cattle-cart to fetch the black mare (six years old), as she could not place her foot on the ground; and another man to lead the grey a distance of fourteen miles. The cart was backed to the door of the loose-box, and the black mare had to be assisted into it by half a dozen men, though she managed to stand fairly well during the journey, being well bolstered up each side with straw. She was carted to a field by the side of a brook, pushed out there, and left for the night, placing a peck of corn on the grass to amuse her. The grey managed to arrive at the

stables about midnight. On the following day I had the latter taken into the forge, and freely applied the iron, afterwards dressing the foot with a stimulating liniment. On the next day I treated the black mare to a similar operation in the field. In eight weeks the grey was sold, and put to light work on the road. She has remained sound ever since; but the black had a second application of the iron. She became sound for work in fourteen weeks from the day of purchase, was sold to Smithers & Sons of this town, and to my knowledge has never been off work from that day to the present date. Facts are stubborn things, and speak for themselves. I have now under treatment a grey mare—if possible a worse case than my black mare—purchased a fortnight since from one of the tramway companies, and which I hope to cure by the same treatment. If you can find space for this statement in your valuable journal you will oblige

Yours truly,

W. FURNIVALL (late "NEMO").

Kingston-on-Thames, *Sept. 7th*, 1878.

[Of the utility of the actual cautery in the treatment of fistulæ and sinuses, there can be no doubt whatever. In the treatment of cervical and dorsal abscess and sinuses, it has proved a most prompt, successful and humane remedy in our hands, when employed skilfully and the iron was at a high temperature.—ED. *V. J.*]

OUR FEES.

To the Editor of the VETERINARY JOURNAL.

SIR,—Since public attention has been directed of late to the subject of Physicians' Fees, I deem this a most opportune time to embrace the subject of our own professional fees; and with your kind permission I will do so through your pages, knowing, as I do, how thoroughly earnest you are to advance the interest of your professional brethren, and particularly those on whose behalf I speak, and in whose ranks I stood for some twenty years—"the rural members of the profession." I am not speaking for the more favoured practitioners of large towns and cities, but the hard-working men in small towns or villages, surrounded by legions of quacks of all denominations. The usual charge for journeys is *6d.* per mile. Now, the question is, Is this fair? My answer is No, and emphatically *No:* the fee should be *1s.* per mile. Many years ago, when I first embarked as a rural practitioner, ponies, cobs and horses could be obtained at *half* the price now charged. Your cart-builder and wheelwright did not charge so much by 30 per cent., wages have ascended gradually, provender has also made an upward movement, and that important firm—the chemist and druggist—has notified the upward tendency of his indispensable agents; everything has gone up in the market, and still we have the same fees—(may I say fees, or must I say prices or charges?) Of course, when making journeys, we can work in a visit or two; but the question is, What is a fair mileage for a visit? If it be *1s.* by day, it should be *1s. 6d.*, I think, by night.

Then medicine. Here again we have the same price as before the rise, and farmers are very close for cheapness. They prefer Day, Son, and Hewett's "Stockowners' Chest of all sorts for everything"; and then, when in a fog, they fall on the local veterinary surgeon, and at a time generally when there is neither a chance for the veterinary surgeon nor his patient. Now, suppose a case such as this: what fee would recompense a man for a ten-mile journey, and at night?

With regard to the mixtures, there should be a general fixed professional charge—say at per dose; the mixture may be four or six doses.

XUM

Then, again, as to examinations for soundness, the fee should always be one guinea; but if a client asked for an inspection only, without certificate, then the fee might be 10*s.* 6*d.* I made a journey last year of nearly twenty miles by rail; my fare was 6*s.* 6*d.* return. At the end of the year I sent my account, and charged one guinea fee and rail—*i.e.*, £1 7*s.* 6*d.* The client refused, as Mr. So-and-So and Mr. So-and-So only charged him 10*s.* 6*d.* fee for examination; and he would not pay more unless the county court directed. Now, 10*s.* 6*d.* is quite inadequate, even as a fee for submitting to the dealer's abuse for spoiling a good "deal."

In the county court, again, some reform is needed. I was on a trial by subpœna; I arrived at court at 10 a.m., the time indicated on the subpœna; the court opened about eleven o'clock. I was dismissed at about 4 p.m.; the cost being taxed, I had 10*s.* 6*d.* offered me by the solicitor, which *I flatly refused*, and for aught I know it is there now; and I observed in the "Cow case" reported in your last issue, *only* 10*s.* 6*d.* was allowed the professional witnesses. Why, it is preposterous to treat and pay professional men with such a niggardly spirit!

I remember the case wherein the late Mr. Field, jun., stood the ground and obtained the higher fee.

Whilst discussing fees, I think I might mention "black mail." I shall be so delighted to know that the practice of "feeing" coachmen, grooms and bailiffs has ceased from our midst; for where this "feeing" is now done, there is but little chance for our needy yet skilful and clever neighbour. The welfare of the stud and stock generally is entrusted to these *important* men—the coachman, groom, bailiff, etc.—in most instances; but the day may come when the *owners* will occupy themselves more with their *own interest*.

To step aside, I think it is time a fixed form of certificate as to soundness should be introduced. Here is an outline of one handed to me by a client last week:—

"This is to certify I have this day examined a six-year-old brown gelding, belonging to 'Mr. Brown,' and I do not discover any unsoundness.

"Signed, M.R.C.V.S.L."

Now, in a court of law Mr. M.R.C.V.S.L.* would look rather scared when Mr. Lawyer Sharp de Twistem smilingly asked him—'Are you a qualified man? You acknowledge you are a qualified veterinary surgeon? You examined the horse in question? Now, sir, as a qualified professional man, I ask you the result of your examination: Was the animal sound or unsound?' *Ans.* 'I do, did not detect——' 'Stop, stop, sir. Keep to my question. I did not and do not ask you what you did or did not detect. Answer my question, Yes or No: The result of your examination, sound or unsound?'

The form I have adopted, and by the advice of an old legal friend fond of the chase, is this:—

"I certify that I have this day examined a by the order of , and belonging to , and I pronounce to be sound, and seven years old this year.

"Signed, M.R.C.V.S.

"N.B.—Particulars, marks, and remarks (if any) on the other side."

In conclusion, I do hope that a scale of fees may emanate, through your assistance and that of the medical associations; so that all members may be uniform in their charges, and this will do much towards bringing the body together on a friendly footing. I have not mentioned contract charges: I leave this open, as also operations.

Consultation fees should certainly be fixed: a F.R.C.V.S. should have

* (?) What does this mean?

some inducement; the fees should be fair, and the public should know them. The value of consultation in serious cases no sensible man will dispute; and what has tended more to the elevation and dignity of the medical profession than the consultations? Then let us emulate them. The public will then soon recognise us not only as professional and scientific men, but as gentlemen.

"I am, etc., yours truly,

"ARTEMUS SECUNDUS."

THE EAGLEY MILK-POISONING CASE.

To the Editor of the VETERINARY JOURNAL.

SIR,—Seeing another letter in your last issue from Dr. Robinson, in reference to the above, we feel reluctantly compelled to advance an explanation in self-defence.

It is not surprising that Dr. Robinson should endeavour to vindicate the opinion he formed, but we are sorry to see he finds it necessary to strain every point in order to make it agree with his expressed opinions. We do not care to recapitulate the whole case. There is abundance of evidence, in all the published accounts, that Dr. Robinson's opinion from the first was that the epidemic in Eagley arose from what he erroneously supposed to be diseased milk given by a particular cow. In this he was totally and entirely in error, as clearly shown in our letter, page 63 of last July's *Journal*.

In reference to the milk having been stopped previous to our attendance at Eagley, the Doctor is again incorrect; since the milk was being sold that very day. This fact we ascertained when calling upon various people who had that day purchased it from Mrs. Kershaw's farm, at Eagley; and only in a few cases had it been refused, but not from any official source; in proof of which we quote a paragraph from the letter of the Surveyor and Inspector of Nuisances, page 64 (July), to this effect:—

The Chairman acting upon this (the letter to him, on the evening of the 8th February, from Messrs. Taylor, Greaves, and Locke), prohibited the sale of the milk from the farm that very night; this was the first and only official notice that had been given to Mrs. Kershaw. The night referred to was the 8th February, and the notice, if it should so far have escaped Mr. Hoyle's memory, was written by Messrs. Greaves, Taylor, and Locke, in Mr. Mollineux's surgery in Bolton, in Mr. Hoyle's presence, and with his assistance and entire approbation, as he concurred with us that it was essential that the sale of the milk should be stopped at once. This letter Mr. Hoyle undertook to deliver to the Chairman of the Turton Local Board that night; so how far he can substantiate Dr. Robinson's version it is difficult to conceive.

With regard to the imputation relative to Mr. Greaves having obtained the signature of Mr. Hoyle "in anything but a straightforward manner, and is in the main untrue, and therefore unreliable and useless," as Dr. Robinson states, this is undoubtedly another error, as Mr. Greaves distinctly asserts that he read over the certificate carefully to Mr. Hoyle, and then asked him "Is there anything in this that is untrue?" Mr. Hoyle's reply was, "It is correct; I have no hesitation whatever in signing it."

Yours, etc.,

THOMAS GREAVES, F.R.C.V.S.
THOS. TAYLOR, F.R.C.V.S.
SAML. LOCKE, M.R.C.V.S.

[This correspondence having reached a stage at which it can no longer be deemed useful or instructive, must now be considered closed. —ED. *V. J.*]

TO CORRESPONDENTS.

WE shall feel extremely indebted to any of our numerous readers in India, Ceylon, or South Africa if they would forward us one or more elephant's eyes when they have the opportunity, in order that a most interesting point with regard to the histology of the retina in that animal may be fully investigated. The eyes must be quite fresh, and excised carefully from the orbital cavity, squeezing of the bulb being avoided and the optic nerve cut about one-eighth of an inch clear of the bulb. When excised, the eyes are to be placed immediately in a mixture of one part by measure of methylated alcohol and two parts of water. If any other alcohol is used, as being more conveniently obtained, the proportion should be so calculated that the alcoholic strength is the same as in that just recommended. The eyes should remain in the alcoholic mixture for thirty to forty hours. The quantity of the mixture should be in the proportion of half a pint to each eye. They are to be removed directly from the mixture to pure glycerine—the volume of this being about four times that of the eyes. A few drops of strong solution of carbolic acid should be added to the glycerine, to prevent the development of fungi. The vessel containing the eyes should be sealed up immediately.

EDITOR "FOREST AND STREAM."—We regret very much that we cannot accede to your proposal, our exchange list being already full.

CONSTANT SUBSCRIBER.—We have never issued what have been designated "double numbers" at a much higher price than ordinary numbers, nor do we intend to do so. Each number of the *Veterinary Journal* contains about twice as much matter as any other veterinary periodical published in the English language at the same price, and much more than the so-called "double numbers" which were a shilling dearer. One of the objects in view in commencing the *Journal* was to put an end to the "double number" nuisance, and in this it has apparently succeeded.

R. MCLOUGHLIN, BOSTON, MASS.—Two numbers will be forwarded to replace those lost in transit, and measures adopted to prevent future loss, if such be possible.

Communications, Books, Journals, etc., Received.

COMMUNICATIONS have been received from A. E. Dalby, Burnham Market, Norfolk; W. Furnivall, Kingston-on-Thames; R. Ward, London; "Artemus Secundus"; "Constant Subscriber"; R. McLoughlin, Boston, U.S.A.; Editor "Forest and Stream"; J. Cowie, Bromley; J. W. Hill, Wolverhampton; B. C. Tipper, Birmingham; J. Walker, 7th Hussars, Dublin; J. McFadyean, Edinburgh; Baily and Son, Cirencester; W. Johnstone, Manchester.

BOOKS AND PAMPHLETS: Prospectus of the Royal (Dick's) Veterinary College; *J. A. McBride and T. W. Mayer*, Anatomical Outlines of the Horse; *J. Cossar Ewart*, The Life-History of Bacterium Termo and Micrococcus; On the Life-History of Spirillum; Veterinary Diagrams in Tabular Form.

JOURNALS, ETC.: *Mark Lane Express; Farmer; Agricultural Gazette; Der Thierarzt; Revue Vétérinaire; Wochenschrift für Thierheilkunde und Viehzucht; Archives Vétérinaires; National Live Stock Journal; Annales de Méd. Vétérinaires de Bruxelles; Journal of the National Agricultural Society of Victoria; Live Stock Journal; American Veterinary Review; Journal of the Royal Agricultural Society of England; Recueil de Méd. Vétérinaire; Deutsche Zeitschrift für Thiermedicin und Vergleichende Pathologie; Animal World; Home Chronicle; Dublin Journal of Medical Science; Social Notes; Forest, Gun, Rod, and Stream; Field; Practitioner; Students' Journal and Hospital Gazette; Archivio di Medicina Veterinaria; Il Medico Veterinario.*

NEWSPAPERS: *Melbourne Age* (two copies); *Globe; Cambridge Express; Birmingham Daily Gazette; Australasian; Scotsman; Broad Arrow; Army and Navy Gazette.*

All Communications, Books for Review, Advertisements, etc., must be addressed to the Publisher, 20, King William Street, Strand, W.C.

Morbid Specimens are to be forwarded addressed to the Editor, "Veterinary Journal" Brown Institution, Wandsworth Road, London.

Communications must be accompanied by the name of the writer, whether he may desire to have it published or not. Anonymous letters and articles cannot be inserted. The Editor does not hold himself identified with the views or opinions expressed by Contributors. Communications for insertion in the next number must arrive on or before the 15th of the present month.

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